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A CONSIDERATION OF LASHLEY'S THEORY OF THE EQUIPOTENTIALITY OF CEREBRAL ACTION*

From the Psychological Laboratories of Clark University

WALTER S. HUNTER

In the present discussion I wish to consider the validity of the general theory of cerebral action which Lashley has formulated from his extensive experiments upon the effects of cerebral lesions. These experiments, carefully planned and executed over a period of some thirteen years, present us with a large amount of data whose validity it is not my intention to question. The problem in which I am here interested is rather one of interpretation. Do the data compel us to revise our fundamental conceptions of cerebral action, or do they but add new and valuable details to our knowledge, details which are consistent with current neural theories?

Everyone familiar with the difficulties which are involved in the analysis of brain functions must realize that no theory of these processes rests at present upon a factual basis which is any too adequate. It is impossible to observe directly where nervous impulses go and how they interact. It is impossible to detect changes in synaptic resistances. In the analysis of the neural control of behavior, we must proceed by inferences based fundamentally upon the known anatomical facts, upon the verifiable data of behavior, and upon such information (for example, electrical) as may otherwise be gathered upon the peripheral nerves. By such means students of neural processes have gradually built up the theory that behavior is controlled by stimuli arousing neural impulses which pass over reflex arcs and so excite muscular and glandular responses. The central nervous system is not regarded as a mere conductor of such impulses, but also as an integrator of them. To this conducting and integrating function current theory also adds the probability of some general neural capacity or plasticity (call it what you will) which sets the limits of accomplishment by the individual. With such a total theory Lashley contends that his experimental data are inconsistent. He therefore proposes a new hypothesis to the effect that the cerebral cortex, and probably also the spinal cord (15, p. 163), functions as a qualitatively homogeneous whole.

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I shall not attempt to summarize Lashley's experimental work. This task he himself has done (15). Herrick (6) also offers a less detailed summary, but one which is accompanied by criticisms of the interpretative point of view. In taking up the consideration of the interpretations which Lashley makes of his data, I shall pass over certain minor items to which exception might legitimately be taken in order that the discussion of the general theory may stand out clearly and distinctly. In approaching this general problem, it is necessary that we analyze the behavior problems which Lashley used with his rats. As a result of this analysis it will, I believe, be clear that the theory of the equipotentiality of neural action is not necessitated by the facts.

Lashley's work has involved the use of three behavior problems: (*a*) the Yerkes discrimination apparatus arranged for light-dark discrimination, (*b*) the inclined-plane box, and (*c*) the maze. The results with these three problems to which I wish to call attention are as follows: I. *Brightness discrimination.* "In normal animals the habit of brightness discrimination is mediated by the occipital pole of the cerebrum (area striata) and by no other part of the cerebral cortex" (14, p. 126). "Learning at normal rate of . . . visuo-motor habits is possible after the destruction of any given part of the cerebral cortex" (14, p. 126). ". . . it seems certain that there is no cerebral localization in vicarious functioning for the visual area" (15, p. 66). Complete decerebration was not done, but the evidence indicates that some cerebral functioning is necessary both for learning and retention (15, p. 65). II. *Inclined-plane box habit.* "The complete destruction of the frontal regions of the cortex results in the loss of the inclined-plane box habit" (13, p. 133). Partial destruction of this area, irrespective of which part is destroyed, does not entail a loss of the habit (13, p. 134). III. *Maze habit.* "1. The capacity to form maze habits is reduced by destruction of cerebral tissue. 2. The reduction is roughly proportional to the amount of destruction. 3. The same retardation in learning is produced by equal amounts of destruction in any of the cyto-architectural fields. Hence the capacity to learn the maze is dependent upon the amount of functional cortical tissue and not upon its anatomical specialization" (16, p. 175). "9. The maze habit, formed before cerebral insult, is disturbed by lesions in any part of the cortex. The amount of reduction in efficiency of performance is proportional to the extent of injury and is independent of locus. 11. No difference in be-

havior in maze situations could be detected after lesions in different cerebral areas, and the retardation in learning is not referable to any sensory defects" (16, pp. 175-176).[1]

In the summary of his book (16, p. 176) Lashley makes the following general interpretative comments: "1. The learning process and the retention of habits are not dependent upon any finely localized structural changes within the cerebral cortex. The results are incompatible with theories of learning by changes in synaptic structure, or with any theories which assume that particular neural integrations are dependent upon definite anatomical paths specialized for them. Integration cannot be expressed in terms of connections between specific neurons. 2. The contribution of the different parts of a specialized area or of the whole cortex, in the case of non-localized functions, is qualitatively the same. There is not a summation of diverse functions, but a non-specialized dynamic function of the tissue as a whole."

The above quotations, although brief, do not, I believe, misrepresent Lashley's theory or the essential basis upon which it rests. Let us proceed, therefore, with our critical analysis. The first point that I would make concerns the sensory control of the three habits concerned. Current theory of cerebral action stresses both the functioning of the projection areas and the general inhibiting, facilitating, or integrating functions of cortical tissue. Lashley's theory does not reject this latter phase of current belief, but it does definitely reject the concept of projection areas with the subordinate notions of reflex arc systems and the control of behavior by the association of various parts of the cortex. In so doing the theory passes lightly over the facts for visual discrimination habits (a point to which I shall come back later) and emphasizes the data derived from the maze. (This point will be quite apparent upon an examination of Lashley's own summary in the volume on *Brain Mechanisms and Intelli-*

[1]S.B. Lindley, in an article on "The maze-learning ability of anosmic and blind-anosmic rats," *J. Genet. Psychol.*, 1930, **37**, 245-267, has found that the number of trials required to master a multiple-T maze is dependent upon the receptors which are functional. Normal rats require 12.3 trials; blinds, 14.5 trials; anosmics, 27.3 trials; partial anosmics, 20 trials; and 11 of 15 blind-anosmics failed to learn in 30 trials. This finding by Lindley is not only in harmony with the multiple stimulus control of the maze response, but it suggests very strongly that the increase in learning-time which Lashley found to accompany an increase in the size of the cortical lesion is partly or wholly due to the elimination of more and more central projection areas as more and more cortical tissue is destroyed. In other words, Lindley's experiments, as well as other experiments which show a multiple stimulus control of the maze response, indicate the incorrectness of Lashley's above statement that "the retardation in learning is not referable to any sensory defects."

gence, pp. 175-176.) Now, if the sensory functions of the cortex are to be studied, it would seem that the first essential would be to know what stimuli control the behavior whose acquisition and retention will presumably be affected by the cerebral lesions. If we are to study the effect of cerebral lesions upon the acquisition and retention of forms of behavior which are not controlled by stimuli, then we must first prove that such forms of behavior exist. It is theoretically possible to create lesions and then to attempt to diagnose the type of sensory control involved, but this method is highly undependable, as the results with the maze habit show. In the case of the visual discrimination habit, the controlling stimuli are specifically known. We have less certain knowledge in the case of the maze habit, but experimental work points to a multiplicity of effective stimuli as opposed to the single definite stimulus which plays the determining rôle in the visual habit. In the case of the inclined-plane box habit we have essentially no information on the sensory control of the behavior involved.

I wish to comment at some length upon the problem of the sensory mechanisms involved in visual discrimination and in the maze habit because I believe that in such an analysis lies the key to the correct interpretation of Lashley's findings. The visual discrimination habit is set up in an apparatus through which the rat runs trial after trial. In so running he encounters from trial to trial the same non-visual stimuli from within the maze and from the environment of the maze. The only stimulus which varies with the location of the correct alley is the light which is shifted from right to left and vice versa. If the experiment is properly controlled the habit when established is definitely known to be controlled by visual stimuli. To be sure, various other stimuli (kinaesthetic, cutaneous, olfactory, auditory, and visceral) are present and probably influence the behavior, but the experimental situation has been so arranged that only the visual stimuli can determine the response to the correct alley. So much for the behavior side. On the cortical side, it is known that there is an area intimately connected with the retinas. The probability therefore is that the complete extirpation of this area will seriously affect the performance of the visually controlled behavior. The results amply confirm this assumption. (Anything less than total extirpation of the visual area would not be expected to eliminate a mere brightness discrimination, whereas such an extirpation might destroy pattern discrimination but only if there had been a control of eye-movements during training and retesting.

Without a control of eye-movements it would be impossible to limit training to a specific retinal area and therefore possibly to a definite portion of the visual cortical area.) Furthermore, if a normal learning rate and a normal retention are affected in some way by the cortex as a whole, we should expect that lesions outside of the visual area would influence in some degree the learning and retention of the habit. Again the results confirm the theory. There is nothing, then, in Lashley's results on visual discrimination which is antagonistic to current general theory.

Let us suppose, however, that the apparatus for visual discrimination were so arranged that many different stimuli varied with the location of the light. Then when the light was shifted from side to side of the apparatus there would also be a shift of these stimuli, e.g., temperature, contact, odor, and noises from the experimenter or from the apparatus. In such a case it is probable that a total lesion of the area striata would not abolish the habit. Indeed it would not be surprising if the effects of cerebral lesions would be the same as those found for the maze habit. It is probable that no single third of the cortex could be shown to be responsible for the control of the behavior in such a case.

In the visual discrimination habit we are too wise to allow the above-mentioned non-visual stimuli to play a rôle in shifting the response from one side to the other of the apparatus. In the maze habit we are not yet wise enough to specify what precise stimuli are involved and when and where they function in the maze. And yet the results show that we are not justified in concluding, as Lashley does, that the maze habit is not controlled by any stimuli but by a central neural engram which unwinds, the figure of speech is mine, like some Victrola record when the rat is placed in the maze.

In order adequately to envisage the maze problem, I must recount a certain amount of the history of attempts to answer the question of the sensory control of the maze habit in rats. This history has been told many times before, but the reader who is not intimately familiar with the literature needs to have the story retold in order that he may have the perspective against which to evaluate Lashley's theory and my criticism.

Watson (18) made the first careful attack on the problem. His essential results are as follows: Nine normal rats learned the maze habit in the light, and thereafter, with exceptions, they could run the maze accurately in the dark. Four blind rats learned to run the maze correctly. Five anosmic rats also formed the habit in the light

and could run the maze in the dark. Two vibrissaeless rats learned the maze response. Two rats with decreased auditory sensitivity had no unusual difficulty with the maze. Two blind rats who had mastered the maze could run it after the soles of their feet had been anaesthetized with ethyl-chloride. One rat who was blind, anosmic, and vibrissaeless learned the habit, but with some difficulty. Watson had thus eliminated one sense field at a time (with the above-noted exceptions) until only the cutaneous, kinaesthetic, and organic fields remained. He therefore concluded that the latter fields were the only essential ones and that the maze habit was essentially a proprioceptively controlled response. It is to be noted, however, that no one rat was deprived of all sensory avenues save proprioception (and that we have no information on errors made). Watson's conclusion therefore overstated the case. The proper conclusion would seem to be that the rat can run the maze correctly when deprived of any one of the following senses: vision, olfaction, normal audition, and normal cutaneous sensitivity. There is the additional conclusion that one rat who was blind, anosmic, and vibrissaeless could learn the habit. There is, however, no evidence that each abnormal rat did not learn in terms of his remaining sensory avenues. It should, further, be pointed out that the numbers of subjects were too small to justify any conclusions based upon the comparisons of learning and retention records under normal and abnormal conditions.

Let us now suppose that Watson had eliminated proprioception from otherwise normal animals. This was logically his next step. It is possible, I think probable, from the above results and others that these rats would have mastered the maze. Watson's line of argument would then have compelled him to conclude that the maze habit is not controlled by any sensory process, since each in turn can be dispensed with. This is indeed the conclusion which Lashley and Ball have drawn some twenty years later.

Following Watson's paper, Carr and Watson (3) reported experiments with the maze which were interpreted as consistent with the theory that the maze is kinaesthetically controlled. These experiments were not sufficient to prove that thesis, but they did lend it support. Vincent (19) published a series of papers of much importance indicating that gross visual, olfactory, and tactual stimuli could influence the maze habit. Bogardus and Henke (1) confirmed these findings so far as tactual stimuli in the normal maze were concerned. The next work of outstanding importance was that of Carr (2), who used normal, blind, and anosmic rats

in a study involving the rotation of the maze. These experiments suggest very strongly that the execution of the maze response is controlled by a great multiplicity of stimuli. Carr writes as follows: "The results of this series of experiments confirm the conclusions of other investigators that the maze habit consists essentially of a tactual-kinaesthetic motor coordination. This act is dependent, nevertheless, both during and subsequent to its development upon a wider sensory situation of which it is a part (2, p. 304).... The maze act and the learning process are much more complicated phenomena than the conclusions of some previous investigators would indicate. The habit does not consist merely of tactual, kinaesthetic and motor elements. Other accessory and conditioning components are also present. Learning does not consist merely of the organization of certain tactual and kinaesthetic stimuli with certain movements. Many other sensory factors are present which release their quota of impulses that must be harmoniously integrated and organized in some fashion adapted to the solution of the problem" (2, p. 306).

In 1920 I published a study (9) on kinaesthetic processes and the temporal maze in which evidence was presented that such processes could not be the necessary basis for the maze habit. The problem was resumed with raccoons in the temporal maze in 1928 (11) and explicitly attacked in a paper on rats (12) in 1929, with the use of bidimensional, tridimensional, and temporal mazes in which the proprioceptive stimuli were equated and so could not afford a basis for differential responses. On the basis of the experiments on the bidimensional and tridimensional mazes it was concluded that where proprioceptive control was impossible the maze habit was controlled by stimuli from other sense fields, stimuli which the rat received as it ran over succeeding units of the maze and through new parts of the environment. The only method so far utilized to control such stimuli is the temporal maze where the rat runs through the same parts of the environment and of the maze during successive units of the response. (This, it will be remembered, is the case in the Yerkes discrimination box except that there a light is shifted from one side of the apparatus to the other as a method of controlling the response.) Another possible method would involve changing all external stimuli from the maze and its environment at each trial. This could be done by interchanging the units of the maze while keeping the pattern constant and by rotating the maze as a whole. [Something very similar to this is reported by Dashiell (4, p. 20).] The temporal maze can be mastered by the rat only with the greatest

difficulty. (During 1930-1931 the *Journal of Genetic Psychology* will carry articles by Hunter, presenting additional material on this problem with rats, and by Gellermann, with rhesus monkeys and human subjects.) The successful execution of the double-alternation–temporal-maze habit cannot be explained in the ordinary stimulus-response terms where specific cues from the maze or the environment of the maze, or the kinaesthetic processes thereby set up, are said to control the behavior. The explanation seems to require the assumption of some symbolic process or some neural engram. The neural engram would be a central control, but it is to be noted that a symbolic process may be peripheral as in the case of human language and as I have elsewhere indicated (7, 8, 10) in commenting upon the delayed reaction. It is an excellent scientific maxim not to appeal to central processes so long as there remains the *possibility* of explaining the given phenomenon in peripheral terms. This is well exemplified in theories of audition and elsewhere. However, such a position is not equivalent to the denial of central neural processes. The insistence is merely that such processes should not be appealed to until peripheral possibilities of explanation are excluded. We know that behavior can be controlled by receptor processes, whereas we still seek to prove a similar possibility for central processes. To be sure, data on behavior justify the assumption of neural changes as a result of training, but they do not justify the assumption that such changes can somehow control behavior in the absence of stimulation.

Let us resume the historical account of the sensory control of the maze response. In 1929 Dennis (5) reported an experiment in which he found that blind, vibrissaeless rats were unable even under conditions of punishment to perfect a simple maze habit upon a purely proprioceptive basis. In normal rats vision supplemented other stimuli where contacts with the walls of the maze were eliminated by punishment.

Lashley and Ball (16), also in 1929, attacked the problem of kinaesthetic control by following to the end the logic of Watson's original paper. They sought to eliminate proprioception by sectioning various portions of the rat's cord at the cervical level. No one rat had all of its fiber tracts severed, but all tracts save portions of the ventral funiculus were presumably severed in the group as a whole. The results indicated that the maze habit in some animals can persist after extensive interferences with the spinal conduction paths. Lashley and Ball conclude in part as follows: "From this it

is argued that the maze habit can not be interpreted as a series of kinaesthetic-motor reflexes but must be referred to some intra-neural mechanism capable of producing an integrated sequence of movements in the absence of directive sensory cues" (16, p. 100).

I have elsewhere (12) written in criticism of this experiment as follows: "I hesitate to accept the authors' conclusions for several reasons: (1) The maze was a simple alternation *rlrlr* maze which would be easily learned and well retained. Therefore any lack of completeness in the severing of proprioceptive conduction paths might well fail to disturb the response by leaving a sufficient basis for executing this simple habit. (2) We cannot be sure that the proprioceptive impulses are confined to the tracts severed in any one rat. Lashley notes this possibility, but does not regard it as probable. (3) Maze habits are influenced by non-proprioceptive stimuli from the environment of the maze and from the maze itself. These stimuli may have played a sufficient rôle in the present simple habit to have supplied the deficiencies created in the proprioceptive processes" (12, p. 507). In addition to these points I would call attention to the fact that the figures listed in Table 1, page 92, of Lashley and Ball's paper indicate disturbances both in normal and operated animals when the maze was darkened. It is true that, since the maze was darkened with a cover and not by turning off the room lights, the results may indicate either a dependence upon vision or a distraction mediated by vision, olfaction, audition, or contact. I believe that the authors pass too lightly over these data when they merely call attention to the similarity between the behavior of the normal and the operated rats. Their statement is as follows: "The animals were disturbed by the presence of the cover and spent much time in pushing against it through the wire netting but their behavior in this respect did not differ before and after operation and their accuracy after operation was in general better than before" (17, p. 95).

A monograph by Dashiell has just appeared in which the conception of the maze habit as controlled by multiple stimuli is further supported. I quote as follows: "It follows, then, that the maze habit becomes established as a system of responses to a veritable complex of stimuli of different degrees of specificity as to their locality" (4, p. 41). "Sensory guidance in maze running is a *complex of stimuli* of various modalities, and operating in different degrees of spatial definiteness from strictly local stimulus-points to general orienting cues" (4, p. 44). In order to account for his own results with a

maze of multiple true pathways, Dashiell offers the following hypothesis: "When a rat first enters the maze there may be set up some kind of kinaesthetic or organic posturing or set (developed in preceding trials), determined by the animal's position when proceeding up the entrance passage way. Then, as the animal traverses the maze and encounters obstacles forcing it to turn right or left, a persisting segment of the initial orientation may inhibit specific stepping movements antagonistic to it and facilitate those consonant with it" (4, p. 69). Undoubtedly some such factor is at work in maze running not only in the case of the maintenance of orientation but also in the guidance of relatively prolonged responses by specific stimuli. The point that interests us here particularly is that Dashiell has not thought it necessary to desert a stimulus-response type of explanation for some intriguing but empty concept of *Gestalt* or other central neural process.

This brings our brief review of the work on the sensory control of the maze up to date. Following my article (12) above referred to, I began an investigation to test the possibility that, when proprioceptive stimuli are equated and when the maze turns are arranged in double alternation, the rat's responses in a spatial maze may be controlled by stimuli received either from new portions of the environment or from the units of the maze over which it runs. This work is now being concluded and will appear in the *Journal of Genetic Psychology* for 1930 or 1931. The present paper is not the place for an adequate presentation of the results. The results show, however, that with blind rats and a double alternation elevated maze whose units are of equal length, the rats are disturbed by the interchange of the maze units. Thus the standard record shows that, after the mastery of the habit, about 10% of the maze runs contain some error. There are nine units in the maze which we may number from 1-9. If the units are arranged in the order 1, 4, 5, 6, 7, 8, 9, 2, 3, 4, but with the same maze pattern, 45% of the runs contain error. If the units are in the order 9, 4, 8, 3, 6, 1, 5, 2, 7, 52% of the runs contain errors. If this same order of the units is tested, but with a buzzer which normally is sounding now made silent, the percentage of runs with errors increases to 69%. Such results lend further definite confirmation to the conclusion that the maze habit is controlled by a multiplicity of sensory cues. They are furthermore consistent with the belief that in the absence of any one cue the control of the behavior shifts to those that remain.

It has been necessary in evaluating Lashley's theory of the equipotentiality of cerebral action to offer a detailed analysis of the sensory control of the maze habit *because the theory rests almost solely upon data gathered on this habit.* If the maze response were controlled by a single sensory field, as is the case with brightness discrimination, then one would expect the results on cortical lesions to reveal some fairly definite localization of the neural control. If, however, many different stimuli are essentially equally effective in the control of the response, then no one cortical lesion should destroy the response, and the ability to form and retain the habit should be decreased roughly in proportion to the amount of cerebral tissue destroyed. As more and more tissue is eliminated, more and more cortical projection areas are removed, and the total integrative function of the cortex is increasingly hampered.

Although, as I have said, Lashley in his final conclusions deals almost entirely with data on the maze habit, it is not to be assumed that he does not see the difficulty created for his theory by the data on visual discrimination. Lashley explains the difference in results for the two habits on the basis of the greater simplicity of the visual discrimination habit. I am unable to determine from a reading of Lashley's account just wherein the difference in complexity lies; but to me the obvious difference in complexity lies on the stimulus side. In the Yerkes box the rat does not manifest any more simple motor behavior than in the maze. On the motor side the chief difference would seem to be that the maze involves a pattern response, i.e., the following of a definite spatial pathway, whereas in the Yerkes box the responses occur without any obvious spatial or temporal pattern. It may be that a wider cortical involvement is present in those responses which are in the form of a temporal or spatial pattern. Even if this were the fact, it would not be necessary without further ado to give up the conception of the brain as an associative and integrative mechanism. With reference to the explanation of the difference between the two habits on the basis of a complexity Lashley says: "The interpretation is not very illuminating but seems preferable to the admission that learning may involve two kinds of nervous activity as strikingly different as those which our data would otherwise seem to reveal" (16, p. 141). It would probably not be impossible to arrange a series of problems differing in complexity on the basis of an increasing multiplicity of sensory cues involved and to test the effect on them of various cerebral lesions. If this were done,

I should expect the results to harmonize with those secured from the maze. (I suspect strongly that some difference in sensory control is at the bottom of the differences between the inclined-plane habit and the maze habit which are the result of cerebral lesions.)

Although the great body of evidence supports the conclusion that stimuli play a leading rôle in the control of behavior, the actual specification of the stimuli involved in the control of many forms of behavior is an exceedingly difficult task. We have seen that this is the case with the maze response, and we may be certain that the analysis of sensory control will be no easier for such forms of behavior as the recognition of their young by birds and for sex behavior. [See Lashley (16, p. 158).] There is no reason as yet to suppose that the difficulty lies more with current neural theory than with our faulty control and analysis of very complex phenomena.

My conclusion is that Lashley's experimental data harmonize with the multiple-stimulus control of the maze habit and the single-stimulus control of brightness discrimination. The data therefore do not demand a radical revision of our current theory of neural action. I think of three possible ways of interpreting Lashley's data. Lesions of the cerebral cortex fail to reveal a localized control of the maze habit because: (*a*) the cortex is equipotential, (*b*) there are many sensory projection areas involved in maze control and the lesions do not affect all of them, or (*c*) the maze habit is controlled by multiple stimuli but the neural integrations are made primarily at subcortical levels. The first possibility is the one that Lashley adopts, whereas I submit that either of the other two possibilities is in greater harmony with the data derived from studies of the sensory control of the maze. Of possibilities *b* and *c*, I feel that *b* is the more probable explanation with rats that have learned the maze prior to cerebral injury. Such a conclusion would, moreover, be in accord with the findings on brightness discrimination. If experimental work, however, should show that the neural integrations involved in the maze habit were made normally at subcortical levels, it would still be unnecessary to throw overboard the theory that reflex arcs were one of the important mechanisms in the control of behavior. I have no particular veneration for the reflex arc theory, and I certainly do not think that reflex arcs function as a simple system of isolated conductors, and yet the general reflex arc theory conforms more nearly to the known facts than does any other.

Summary

Lashley's theory of the equipotentiality of cerebral action is not justified by the known facts concerning the sensory control of the maze habit. A brief history of the experiments bearing upon this latter topic is given, and the conclusion is reached that the maze habit is essentially controlled by a multiplicity of stimuli rather than by a central neural engram as Lashley and Ball unjustifiably infer. Attention is called to the fact that Lashley's theory rests almost exclusively upon data gathered from the maze habit and that the theory fails to harmonize the results secured from this habit with those secured from the visual discrimination habit.

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UNE CONSIDÉRATION DE LA THÉORIE LASHLEY DE L'ÉQUIPOTENTIALITÉ DE L'ACTION CÉRÉBRALE

(Résumé)

La théorie Lashley de l'équipotentialité de l'action cérébrale n'est pas justifiée par les faits connus concernant le contrôle sensoriel de l'habitude du labyrinthe. On donne un court résumé des expériences où il s'agit de celle-ci, et on arrive à la conclusion que l'habitude du labyrinthe est essentiellement contrôlée par une multiplicité de stimuli plutôt que par un engramme neural central comme Lashley et Ball infèrent sans justification. On appuie sur le fait la théorie Lashley est basée presque exclusivement sur des données tirées de l'habitude du labyrinthe et que la théorie ne réussit pas à accorder les résultats donnés par cette habitude avec ceux donnés par l'habitude de discrimination visuelle.

HUNTER

EINE ERWÄGUNG DER LASHLEYSCHEN THEORIE ÜBER DAS GLEICHVERMÖGEN (EQUIPOTIENTIALITY) ZEREBRALER TÄTIGKEIT

(Referat)

Lashleys Theorie über das Gleichvermögen zerebraler Tätigkeit wird durch die uns bekannten Tatsachen bezüglich der sensoriellen Beherrschung der Labyrintgewöhnung nicht bestätigt. Die Arbeit gibt eine kurze historische Darstellung der Experimente über den genannten Gegenstand und führt zur Schlussfolgerung, dass die Labyrintgewöhnung im Wesentlichen eher durch eine Vielheit von Reizen als durch ein zentrales Nervenengramm beherrscht wird, wie Lashley und Ball unberechtigterweise folgern. Man erwähnt, dass Lashleys Theorie fast ausschliesslich auf Angaben beruht, die von der Labyrintgewöhnung gewonnen wurden, und dass die Theorie die von dieser Gewöhnung herstammenden Ergebnisse nicht in Übereinstimmung zu bringen vermag mit denen der Gewöhnung in visuellen Unterscheidungen.

HUNTER

Learning
Also
Growth

THE LEARNING FUNCTION*

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In order to make it possible to write a rational equation for the learning function it is necessary to make some assumptions which are certain to oversimplify the complex phenomena of learning. But oversimplification is the very nature of scientific law. There is probably no law in any science which is not easily violated by introducing any of the practical irrelevancies in which the phenomena are actually experienced. So it is with the law of falling bodies which is violated if you drop them in the air. Even if we know that our assumptions will ultimately be replaced by a scheme of things much more comprehensive it may still serve the purposes of more modest generalization to describe the phenomena of learning quantitatively by an equation that fits more or less with current doctrine and which has a few parameters with physical interpretation consistent with current doctrine.

The two principal variables in learning are (*a*) practice, and (*b*) attainment. Attainment is, of course, an increasing function of practice. Practice may be measured either in terms of repetition or in terms of time devoted to it, and effort is assumed to be constant. Repetitions may be counted as successful acts only, or they may include both successes and failures. Perhaps the most serious assumption in current learning theory is that learning consists in a lot of separable acts and that some of them are right and others are wrong. In the present analysis we shall also make that assumption, namely, that learning consists in a series of separable acts, that some of these are counted as successful and that others are counted as errors. Each act whether right or wrong will be counted as a unit of practice.

Attainment can also be expressed in various ways, but we shall adopt here as a measure of attainment the probability that an act will be counted as successful. This measure of attainment is pro-

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portional to and consequently for our purposes synonymous with the number of successful acts per unit time. We now have a part of our schema. The animal performs a series of acts toward a certain goal, and with repeated attempts the proportion of successes increases. At the start the probability of a success is low. With repeated attempts this possibility rises toward unity. But we shall make a reservation about the act. In the early stages of learning the subject tries as hard perhaps as in the later perfected stages, but he does not produce as many overtly countable acts. We shall assume that the acts initiated in his attempts to succeed are at least as numerous then as later but that the incipient acts are lost as failures even before they reach sufficiently tangible form to be counted overtly as attempts or as complete acts. We assume, in other words, that the number of acts initiated per unit time is the same for the novice as for the advanced learner provided that effort remain constant. Some of these initiated acts are lost in confusion and tension without ever reaching overt form. Others which do reach overt form are counted as failures. Some reach overt completion and are counted as successes. Our assumption here is that the rate at which acts are initiated is directly proportional to effort, which is assumed to be constant.

At any one moment of time all the acts which the learner is at all likely to initiate in his effort toward a goal may then be arbitrarily divided into two categories, those that lead to success and those that lead either to overt or implicit error. Our classifications so far may be summarized as follows:

s=total number of acts which the learner is likely to initiate and which would be credited as successes.

e=total number of acts which the learner is likely to initiate and which would lead to error or failure or loss of time.

p=probability that the act initiated at any moment of time will lead to successful completion.

q=probability that the act initiated at any moment of time will lead to error or loss of time.

Then, clearly,

$$p=\frac{s}{s+e} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [1]$$

$$q=\frac{e}{s+e} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [2]$$

and $ps+pe=s$ . . . . . [3]

The object of the learner is, of course, to reduce the errors e and to raise the proportion p. We make here no assumption whatever about the mechanism by which errors become eliminated. The fact is, of course, that they disappear. We say that a subject is superior if he profits by his mistakes so that errors are not repeated over and over again. The good learner is one who never repeats an error, and the perfect learner is the hypothetical animal that beats the game by anticipating the erroneous outcome of an initiated act so that he never makes an overt error. If one subject makes an error twice, on the average, before it is eliminated and if another subject in the same situation makes an error three times, on the average, before it is eliminated, then the first subject will evidently make faster progress. He profits more quickly from his mistakes. Both subjects will sooner or later eliminate all errors so that the value of p approaches unity.

Analytically this looks very much like an urn problem. Let all the probable or plausible acts of a learner be represented by white and black balls in an urn. He is drawing them out, for the present, one at a time, and he draws them out at a uniform rate. At any moment there is a certain probability p that the ball which he draws will be white and a certain probability q that it will be black. If it turns out to be black, then there is the probability k that it will be eliminated from the urn and the probability $(1\text{-}k)$ that the black ball will be returned to the urn.

p=probability that he will draw a white ball (success)

$q=(1\text{-}p)$=probability that he will draw a black ball (error)

kq=probability that he will draw a black ball and that it will be eliminated (thrown out of the urn)

$q(1\text{-}k)$=probability that he will draw a black ball and that this error will be repeated in future attempts. (He throws the ball back in the urn again.)

For a subject who makes each mistake twice before eliminating it the constant $k=.5$ because, on the average, he will return as many black balls to the urn as he throws out of it. For another subject who makes each mistake four times before it disappears the constant $k=.25$. This means that when a black ball is drawn for the fourth time the learner withdraws it from the urn instead of returning it to the urn. Of course, we need not insist that all errors are repeated exactly the same number of times. We are dealing only with the average number of times that a learner repeats an error before eliminating it.

But whenever an error is eliminated it is obvious that the total number of possible errors e is reduced by one. Further if the probability that a particular draw will give a black ball and that it will be eliminated is kq, then this is also the average number of black balls by which e is reduced *per draw*. This enables us to write the differential equation

$$\frac{de}{dt} = -kq \qquad [4]$$

By [2] we have

$$\frac{de}{dt} = \frac{-ke}{s+e} \qquad [5]$$

This equation describes the rate at which the subject is eliminating his errors in terms of the probability k that he will eliminate a committed mistake.

It is interesting to see what common-sense interpretation can be given to different values of k. Suppose that k were zero for some subject. Then he could never progress because, when he makes a mistake, the probability is zero that he will ever eliminate it. Suppose that k were unity for some other subject. That would mean that, on the average, he eliminates every mistake he makes. He never repeats his errors. But now suppose that k were greater than unity. That has a very interesting interpretation. It means that when this subject makes one mistake he not only eliminates that error but also some more errors that he has not yet made. That is the very essense of rational learning. It looks as though we might have a gradation from trial-and-error learning to rational learning represented by the numerical value of k in our differential equation [5].

But so far we have not mentioned the possibility that a successful act has some effect favoring its recurrence. We shall assume not only that overtly committed errors decrease their probability of recurrence but also that overtly successful acts increase their probability of recurrence. We can write a differential equation for successes, corresponding to equation [5] and it takes the form

$$\frac{ds}{dt} = +kp \qquad [6]$$

which by [1] becomes

$$\frac{ds}{dt} = \frac{ks}{s+e} \qquad [7]$$

Here we have used the same constant k as in [5]. We probably are not doing much violence to the truth if we assume that the extent to which a subject profits by his errors is about the same as the extent to which he profits by his successes. At any rate we shall adopt this approximation.

We are treating the total number of *plausible* errors e as a decreasing quantity, and we are treating the total number of *plausible* successes as though it were in effect an increasing quantity. This does not mean that the total number of *possible* successful acts is actually increased. If one act has been repeatedly successful we may simply represent it in the urn by several *duplicate* white balls. This carries the urn analogy by which the probability of repeating that act is increased.

The differential equations [5] and [7] enable us to find a simple relation between the total number of plausible errors e and the equivalent or effective number of successes s which the learner has at his disposal at any particular moment of practice. Dividing [7] by [5] we have

$$\frac{ds}{de} = -\frac{s}{e} \quad . \quad . \quad . \quad . \quad . \quad . \quad [8]$$

and consequently

$$\int \frac{ds}{s} + \int \frac{de}{e} = 0 \quad . \quad . \quad . \quad [9]$$

so that

$$log\ S + log\ e = log\ m \quad . \quad . \quad . \quad . \quad . \quad [10]$$

where m is a constant. This may be written more simply

$$se = m \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [11]$$

In other words, the product of the total number of errors and the *effective* number of successes in the learner's repertoire is a constant. The physical meaning of this constant m is evidently related to the complexity of the learning task. When the learning process is intrinsically complex for the subject the product se is large. When this product is small, the task is correspondingly simple. The constant k is an attribute of the individual learner while the constant m is primarily an attribute of the learning problem or task.

We want to know the relation between practice time t and attainment p. From [3] we may write the following differential equation.

$$p\frac{ds}{dt}+s\frac{dp}{dt}+p\frac{de}{dt}+e\frac{dp}{dt}=\frac{ds}{dt} \quad . \quad . \quad . \quad . \quad . \quad [12]$$

Substituting [5] and [7] in [12] we have

$$\frac{pks}{s+e}+s\frac{dp}{dt}-\frac{pke}{s+e}+e\frac{dp}{dt}=\frac{ks}{s+e} \quad . \quad . \quad . \quad . \quad [13]$$

and collecting terms it becomes

$$(s+e)^2\frac{dp}{dt}=k\ (pe+s-ps) \quad . \quad . \quad . \quad . \quad [14]$$

The relation between p and t is then

$$\int\frac{(s+e)^2dp}{s-ps+pe}=k\int dt \quad . \quad . \quad [15]$$

In order to perform the integration, the variables s and e may be stated first in terms of the individual learning constants k and m and the desired variables p and t. This can be done as follows. From [3] we have

$$e=\frac{s-ps}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [16]$$

and from [11] we have

$$e=\frac{m}{s} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [17]$$

Solving [16] and [17] simultaneously for s and e, we get

$$s=\sqrt{\frac{mp}{1-p}} \quad [18]$$

and

$$e=\sqrt{\frac{m(1-p)}{p}} \quad [19]$$

The numerator of [15] contains the factor $(s+e)^2$ which is evaluated in terms of [18] and [19] as follows.

$$(s+e)=\frac{\sqrt{mp}}{\sqrt{1-p}}+\frac{\sqrt{m(1-p)}}{\sqrt{p}} \quad . \quad . \quad . \quad [20]$$

which simplifies so that

$$(s+e)^2=\frac{m}{p\ (1-p)} \quad . \quad . \quad . \quad . \quad . \quad . \quad [21]$$

The denominator of [15] can also be evaluated in terms of the individual learning constants k and m and the desired variables p and t as follows.

$$s - ps + pe = \frac{\sqrt{mp}}{\sqrt{1-p}} - \frac{p\sqrt{mp}}{\sqrt{1-p}} + \frac{p\sqrt{m(1-p)}}{\sqrt{p}} \qquad [22]$$

which simplifies so that

$$s - ps + pe = 2\sqrt{mp\,(1-p)} \qquad [23]$$

Equation [15] may be rewritten in more convenient form by substituting [21] and [23]. After simplifying, it takes the form

$$\int \frac{dp}{p^{\frac{3}{2}}(1-p)^{\frac{3}{2}}} = \frac{2k}{\sqrt{m}} \int dt \qquad [24]$$

Here we have a relation between the two variables p and t of the learning curve and the two constants k and m.

In order to facilitate integration, make the substitution $p = sin^2\phi$. The limits of p and $sin^2\phi$ are the same. Then [24] becomes

$$\int \frac{d\phi}{sin^2\phi\, cos^2\phi} = \frac{k}{\sqrt{m}} \int dt \qquad [25]$$

$$= \int csc^2\phi\, sec^2\phi\, d\phi = \int (1 + cot^2\phi)\,(1 + tan^2\phi)\, d\phi$$

$$= \int (2 + tan^2\phi + cot^2\phi)\, d\phi$$

$$= tan\phi - cot\phi + C = \frac{kt}{\sqrt{m}} \qquad [26]$$

But from the substitution it follows that

$$tan\ \phi = \sqrt{\frac{p}{1-p}}$$

$$\text{and } cot\ \phi = \sqrt{\frac{1-p}{p}}$$

and hence [24] becomes

$$\frac{2p-1}{\sqrt{p-p^2}} = \frac{kt}{\sqrt{m}} + z \qquad [27]$$

in which z is a constant of integration. This is the desired learning curve equation and it is shown in Figure 1.

Properties of the Learning Curve. When $t=0$, $p=p_0$. From this we find the constant of integration for then

$$\frac{2p_0-1}{\sqrt{p_0-p_0^2}}=z_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [28]$$

so that the equation may be written in the form

$$\frac{2p-1}{\sqrt{p-p^2}} = \frac{kt}{\sqrt{m}} + \frac{2p_0-1}{\sqrt{p_0-p_0^2}} \qquad [29]$$

in which p_0 is the probability of success when $t=0$.

When $p = 0$ it is clear that $t = -\infty$. In so far as equation [27] really represents the learning process we must therefore conclude that no learning can take place if the process starts with nothing. In other words, there must be some small amount of attainment by native endowment in order that learning may proceed at all and this agrees with psychological common sense. The curve is asymptotic to the base line.

When $p = 1$ it is clear that $t = +\infty$. This is the situation of absolute perfection which is attained according to this theoretical formulation only with infinite practice. This also agrees with the common observation that learning is asymptotic to the physiological limit.

But if the curve is asymptotic to the base line and also to $p=1$, then there must be an inflection point. This may be investigated by equating the second derivative of p to zero. From [27] we have

$$\frac{dp}{dt} = \frac{2k}{\sqrt{m}}(p-p^2)^{\frac{3}{2}} \qquad [30]$$

and, for the second derivative,

$$\frac{d^2p}{dt^2} = \frac{3k}{\sqrt{m}}(p-p^2)^{\frac{1}{2}}(1-2p) = 0 \qquad [31]$$

which is equated to zero in order to locate the inflection point. The values $p=0$ and $p=1$ have already been described as asymptotes. Equation [31] is true when $p=\frac{1}{2}$ and it is therefore clear that the learning curve has an inflection point at that level for the particular assumptions with which we started. The inflection point is indicated in Figure 1 by a small arrow. But it will be shown that the inflection point can be found at other values of p and especially for values lower than $\frac{1}{2}$ under certain conditions.

That the curve in the simple form of equation [27] is symmetrical in the same sense in which the $\Phi(\gamma)$-curve is symmetrical is seen

in the fact that p and $(1—p)$ are interchangeable in [30] without altering the slope of the curve. The symmetry may be shown formally as follows: If the curve of equation [27] is symmetrical in the sense in which the $\Phi(\gamma)$-curve is symmetrical (the integral of the probability curve), then we should place the origin at the value of t at which $p=.5$. This is accomplished by letting the constant c in [27] be zero. Then

$$\frac{2p-1}{(p-p^2)^{\frac{1}{2}}} = \frac{kt}{\sqrt{m}}$$

Now if we substitute $(1—p)$ for p in the above equation (see Figure 1), then we should have the same value for t with sign reversed. This statement is easily verified and hence we see that the learning curve is symmetrical for the simple conditions that have been assumed.

To Determine the Constant k and m for Experimental Learning Curves. The simple form of the learning curve represented by equation [27] can be easily rectified. We may write it in the form

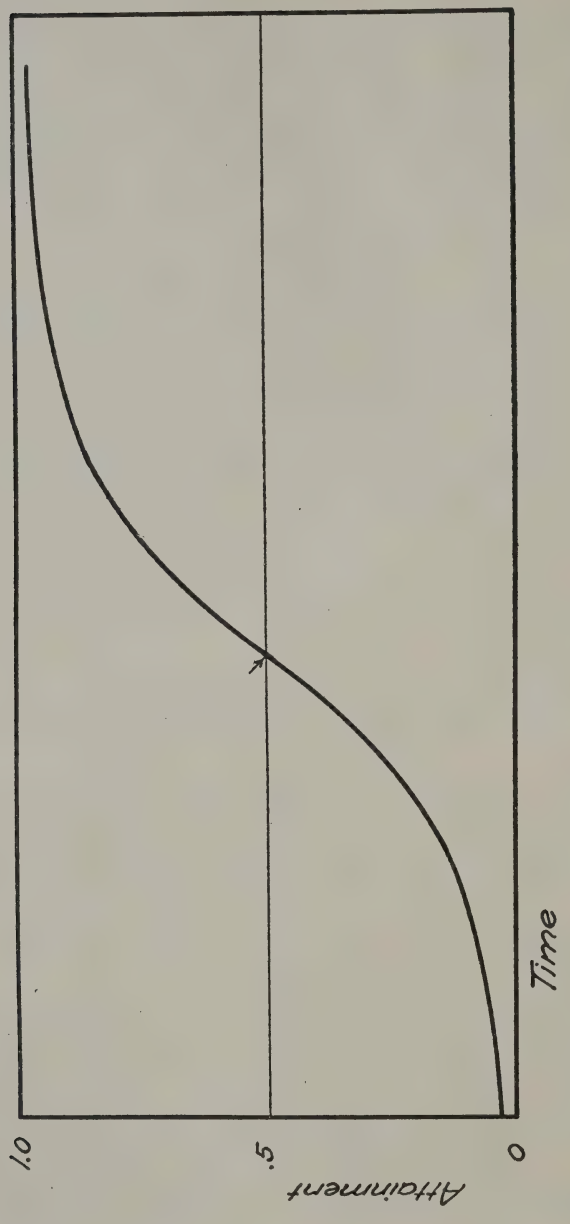
$$\frac{2p-1}{\sqrt{p-p^2}} = \left(\frac{k}{\sqrt{m}}\right) t + c \qquad [32]$$

which is linear in

$$\frac{2p-1}{\sqrt{p-p^2}} = z, \text{ and } t.$$

In order to facilitate the calculation of k and m for experimental data, Table 1 has been prepared. One enters this table with the experimental value of p and one obtains directly the value of z. These values of z are plotted against t, and from [32] it is clear that the slope is $k/\sqrt{m}$ while the z-intercept is the value of z when $t=0$ as shown in [28]. From this intercept and Table 1 the best fitting value of p_0 may be ascertained.

It is apparent in [32] that the constants k and m cannot be isolated in a single learning curve but the separation might be effected if an experiment were to be set up as follows. We are assuming that k is primarily an attribute of the individual learner and that m is primarily an attribute of the task. This distinction is not absolute, but it is probably a reasonable separation. If, for example, two subjects should undertake to learn the English translation of a list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Time

of fifty Chinese characters and if these two subjects have comparable cultural backgrounds, then it would seem reasonable to say that the two tasks are similar in complexity. Progress would not necessarily be the same because one of them might profit more quickly from his errors and he might recall the translations better than the other subject. But this difference between them is described by their different constants k. If we allow in this manner that k describes the learner while m describes the task, then their separation might conceivably be effected experimentally.

Let there be five tasks of the same general kind but of different degrees of complexity. Let there be ten learners all of whom proceed to master the five tasks under similar experimental conditions. Let the experiment be carried out only after the learners have become thoroughly familiar with the kind of material involved. Then we shall have fifteen constants to determine, namely five constants m, one for each of the five tasks, and ten constants k, one for the learning ability of each subject. But since there are five tasks and ten learners, we should have fifty separate learning curves. Let the complexity of one of the tasks be taken as unity so that for example $m_1=1$, then we shall have fourteen unknowns but fifty learning curves. It should now be possible to ascertain whether the fifty curves can be described with equation [27] by assigning one value of k to each subject regardless of the task and one value of m to each task regardless of who learns it. Then, if we know the learning constant k for a person and the complexity m of a task, it should be possible to predict the course of his learning curve if his motivation were not unduly accelerated or retarded by any special conditions. Such an experimental verification has not yet been attempted.

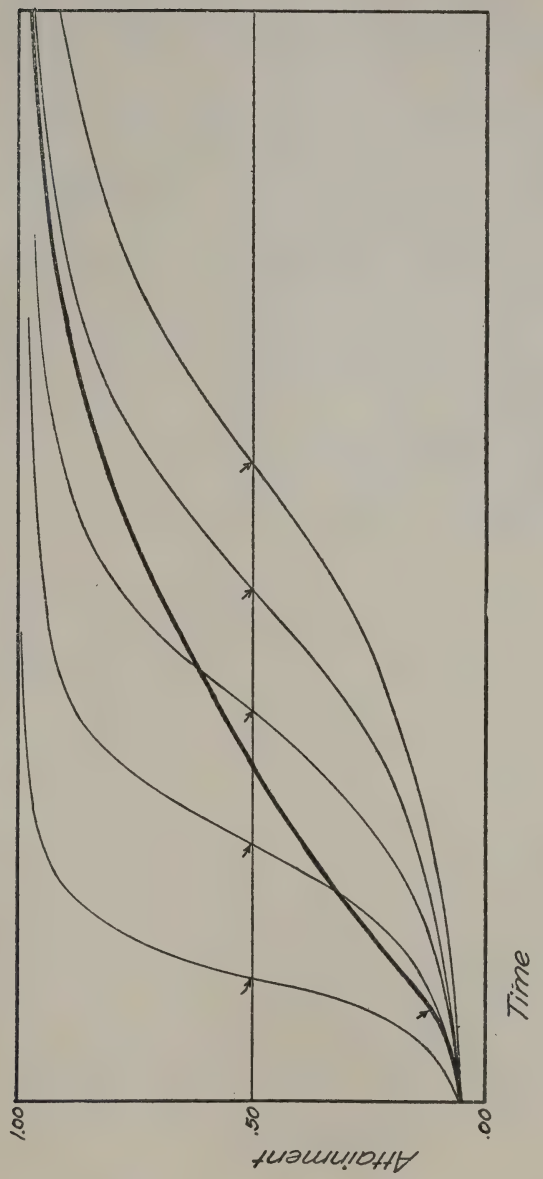
As we have formulated equation [27] it has been assumed that there is one draw from the urn (one act) per unit time. This is not a convenient unit for experimental work. In the experimental verification of the equation it is probably best to define the unit of time as one tachistoscopic exposure or one reading of the learning material. The learning constant k will then be the average number of countable or plausible errors that the subject eliminates for each presentation of the material, i.e., the average number of errors that he eliminates per unit time.

Homogeneous and Heterogeneous Learning Elements. So far we have considered the learning problem as though all of its elements were equally complex and equally difficult to master. Such situa-

tions can be arranged in a laboratory where all of the items are for practical purposes equally difficult to memorize, but in most practical learning situations this simple homogeneity of the learning elements is not found. It is more often the case that some of the elements are, as elements, simpler than other elements in the same task. That is true, for example, in learning to typewrite. Some associations or movements are more complex than other elements. We shall refer to such tasks as heterogeneous in their elements.

If, in the early phase of learning, the simple elements are mastered, leaving the complex elements to be mastered later, then we have in effect a variation in the complexity of the heterogeneous task as learning proceeds. Such learning starts with a relatively low value for m which increases as practice goes on to the more complex parts. The complexity of the task is then a function of time. In order to show the effect of heterogeneity on the shape of the learning function the curves of Figure 2 have been drawn. Here we assume, for simplicity, that a heterogeneous task is a composite of five homogeneous tasks that differ in complexity. If each of these homogeneous tasks were learned separately we should expect them to be symmetrical forms like equation [27], and as shown in the five symmetrical S-curves of Figure 2. Since they differ in complexity so that $m_1 < m_2$ it is clear that the simpler one will approach perfection sooner than the complex one. Now let these five homogeneous tasks be combined into a single heterogeneous task. What happens to the learning curve? We are assuming that the performance as measured in the composite task is equally weighted with each of the five homogeneous components. The learning curve for the composite heterogeneous task is shown in Figure 2 by the heavy line. It is obtained by merely averaging the five component curves.

Inspection of Figure 2 shows immediately that the effect of heterogeneity of the learning task is to lower the inflection point below the value $p=.5$. We conclude, therefore, that a homogeneous task gives a learning curve with its inflection point at $p=.5$ so that the curve is symmetrical. These inflection points are shown in Figure 2 by small arrows. A heterogeneous task gives a learning curve with its inflection point in the lower half of the curve. This theoretical finding agrees with experimental observation. In my own experiments in 1916 (1) on typewriter learning I found that the learning curve is S-shaped when plotted against time and that its inflection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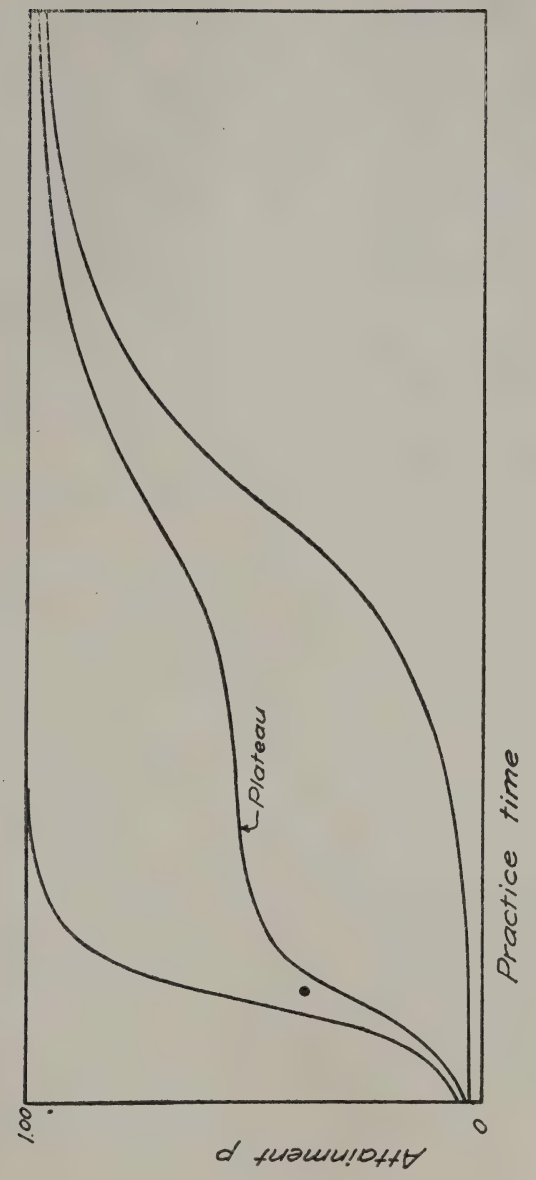
point is at one-third of the physiological limit. Several later studies of the learning curve in this form have confirmed this finding that for heterogeneous tasks the inflection point is in the lower half of the curve.

Notice also in Figure 2 that the heavy curve has the appearance of the typical learning curve which is usually shown with negative acceleration. The short lower part which has a positive acceleration is often missing because the experimental data do not often cover that early range in the learning process.

If the heterogeneous task contains essentially two components, one rather simple and the other very much more complex, then we may expect the learner to master the simple one almost completely before he really begins to make progress on the complex component. This is illustrated in Figure 3 by separating the two component homogeneous learning curves. The effect of averaging them shows a very familiar phenomenon in learning, namely, the plateau. We have here another contact between theory and experimental observation. If our analysis is correct, the slope of the first part should be steeper than that of the second part because of their differences in complexity m.

If the interest of the learner should lag during practice, our analysis would represent this reduction in drive by a lower rate of innervation or by a slower rate at which the balls are drawn from the urn in the urn analogy. Since the constant m is assumed to be at least primarily an attribute of the task, we leave it unaffected by the learner's waning interest, but the rate at which he eliminates errors will, of course, be reduced. Hence the constant k will be reduced by any lowering of incentives or interest on the part of the learner. In such a situation the constant k is a decreasing function of time, the exact nature of which is as fortuitous as the infinite possibilities of variation in the incentives. But it is significant to notice that in the fundamental equation [27] a reduction of the constant k, owing to waning interest of the learner, has the same effect on the appearance of the curve of learning as an increase in complexity of the task during the learning. Both of these factors, the waning of interest and the increase in complexity of the task, lowering k or increasing m during practice, have the same effect of moving the inflection point to the lower half of the curve. This is a further check on theory because it is a matter of common observa-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tion that the inflection point in practical learning situations is in the lower half of the curve.

It should be possible to check this theoretical formulation about homogeneous and heterogeneous tasks experimentally. According to our theory a homogeneous task should give a symmetrical learning curve while a heterogeneous task should give a skewed learning curve with an inflection point in its lower half. An experiment could be set up in which some lists would contain both easy and difficult elements, while other lists would contain elements of uniform difficulty. This experimental verification of our theory with homogeneous and heterogeneous learning material of the same kind has not yet been made.

A more complete definition can now be given for the constant k. The learning constant k for a particular subject and for a specified kind of task is the average number of times that he commits an error before eliminating it when the task is of unit complexity. Since motivation is not subject to accurate control, we can only make sure that the incentives are not unduly affected by any special conditions in the laboratory experiment.

The Conditions for Insight in Learning. One of the current problems in learning concerns the interpretation of insight. Here we may look upon a rather sudden rise to perfection as the objective and quantitative evidence of insight although there are other qualitative and really more convincing forms of evidence for it. The sudden rise in the learning curve to the level of perfection may be expected by our fundamental equation under either of two conditions. These conditions are (a) a value of k considerably higher than unity, and (b) a low value of m. Strangely enough, the phenomenon of insight may be found, according to our equation, at the two opposite ends of the scale of learning, namely, (a) in highly rational or conceptual learning (k is large) and (b) in the simplest possible forms of animal trial-and-error learning (m is low).

When the constant k is considerably greater than unity, the physical interpretation is that when the learner makes one error he eliminates a whole set of errors without ever executing them overtly. This results in the phenomena of insight, a sudden approach to perfection. We have seen that the extent to which the constant k exceeds unity is a measure of the extent to which the learning is rational or conceptual, because the fundamental characteristic of

rational learning is the elimination of errors without really making them overtly.

The second condition which may cause the phenomenon of insight is the reduction of the complexity of the task to so low a value for m that the elimination of one black ball from the urn makes the difference between noticeable imperfection and complete learning. Consider the extreme case of a task represented by one white ball and one black one. The probability is about .5 that the response will be correct until the move when the black ball actually drops out of the urn, at which time the performance suddenly becomes perfect. In the latter situation, when m is numerically very small, our equation does not hold because we are then dealing with a discrete series. We have assumed m to be large enough so that the value of p may be regarded as a continuous function of time. When m is small, this condition is no longer fulfilled and the problem becomes a special case of the more general one that has been discussed. The learning curve would then be discontinuous.

Relation between Length of Task and Learning Time. This is one of the well-known problems in the experimental study of learning. While the relation between the length of the task and the learning time has been studied by a number of investigators, there seems to have been no attempt to rationalize the experimentally obtained functions. If our analysis of trial-and-error learning is at all adequate, it would seem to be possible to predict this relation rationally and this we shall attempt to do.

In order to predict the total learning time T it is necessary to take account of at least three factors in addition to those that are represented by the equation for the learning function, namely, (a) the degree of perfection that the experimenter requires for "complete" learning, (b) the attention span of the subject, and (c) the number of items in the list to be learned.

It is clear that approximately complete learning varies considerably with the standards set by the experimenter. For example, it requires less practice to produce one correct repetition than to produce three successive correct repetitions. The standard set by the experimenter is represented in our equation by a theoretical final value of p which is close to unity. It can never be as high as unity because that would imply absolute certainty of a correct repetition and that is never attained. We may designate the values as follows:

p_1 = final value of p corresponding to the standard of perfection which is set by the experimenter for complete learning.

$$z = \frac{2p-1}{\sqrt{p-p^2}} \qquad \text{and } Z = \frac{2p-1}{\sqrt{p_1-p_1^2}} \qquad [33]$$

The value for Z may be taken as approximately constant for different lengths of material in our first attempts to rationalize these experimental functions.

At the start of the learning function we must take account of what we shall call the attention span of the subject. If the list of nonsense syllables is relatively short, then the subject can span the entire list so as to give a correct reproduction after only one exposure of it. The length of task which the subject can span successfully in the time required merely to perceive it we shall call his attention span. The relation between length of task n and the total learning time T starts with this minimal length of task.

If there are n paired items in the list, then it is clear that the total number of possible correct associations is n and the total number of possible incorrect associations between a stimulus symbol and a response is $n\ (n-1)$ or approximately n^2. But owing to the attention span of the subject there remain virtually $(n-a)$ incorrect associations for every stimulus symbol. Hence there are n correct associations to be mastered and there are about $n\ (n-a)$ possible incorrect associations. In the urn analogy the situation immediately after the start is as though there were n white balls and $n\ (n-a)$ black balls in the urn. Hence

$$s = n \qquad [34]$$

$$e = n\ (n-a) \qquad [35]$$

and $$m = se = n^2\ (n-a)$$

or $$\sqrt{m} = n\sqrt{n-a} \qquad [36]$$

The general equation [27] for the learning function can be written from [33] as follows for the moment of complete learning

$$Z = \frac{k}{\sqrt{m}}\,T + z_0 \qquad [37]$$

in which

$$Z = \frac{2p_1-1}{\sqrt{p_1-p_1^2}}$$

$$z_0 = \frac{2p_0 - 1}{\sqrt{p_0 - p_0^2}}$$

p_1 = final value of p

p_0 = initial value of p

T = complete learning time between the limits of attainment p_0 and p_1.

Since Z and z_0 are here regarded as constants, they may be contracted into

$$C = Z - z_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [38]$$

so that

$$C = \frac{k}{\sqrt{m}} T \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [39]$$

which by [36] becomes

$$T = \frac{c}{k} n\sqrt{n - a} \quad . \quad . \quad . \quad . \quad . \quad . \quad [40]$$

This is the desired relation between the number of items in the list and the total learning time. It has two parameters, namely c/k and the perceptual adaptation constant a. It is conceivable that this adaptation constant may sometimes be negative. If, for example, some time elapses before the subject gets the idea of the task then the constant may be negative. It may be expected ordinarily to be positive in the form of what we have called the attention span.

Equation [40] is only approximately correct even if the general learning equation [27] should prove to be substantially right. The rationalization of the adaption constant in equation [36] can be only very approximate. It is an arbitrary decision to regard the values of z_0 and Z as constant for lists of different length, but these approximations may be close enough at the present stage of rationalization of the problem of learning. The formulation of equation [36] may be varied in a dozen different ways and we have used one of these that seemed simple and fairly reasonable. However, many of the different formulations that are possible become identical for large values of n. If z_0 and Z are allowed to vary for different

lengths of task, then equation [40] becomes much more complicated. With the present form of equation [40] we shall conclude that the total learning time varies approximately as the 3/2 power of the length of the list, especially when the lists are relatively long.

Equation [40] may be modified so as to give the approximate relation between the number of items in the list and the number of repetitions required for learning it if we are willing to write

$$T = n R t_e \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [41]$$

in which

T = total learning time
n = number of items in the list
R = number of repetitions required for complete learning
t_e = time of exposure for each item in the list.

Since t_e may be adopted as the unit of time we may rewrite [41] as

$$T = nR \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [42]$$

Substituting [42] in [40] we have

$$R = \frac{c}{k}\sqrt{n-a} \quad . \quad . \quad . \quad . \quad . \quad . \quad [43]$$

which means that for large values of n the number of repetitions for a homogenous task varies approximately as the square root of the length of the task. This equation can be only approximately true. However, we are perhaps safe in concluding that when T is plotted against n we should have a curve of positive acceleration, and that when R is plotted against n we should expect a curve of negative acceleration.

TABLE 1
VALUES OF z FOR GIVEN VALUES OF p

	.00	.01	.02	.03	.04	.05	.06	.07	.08	.09
.0		—9.899	—6.857	—5.497	—4.694	—4.128	—3.713	—3.373	—3.100	—2.867
.1	—2.667	—2.492	—2.338	—2.202	—2.081	—1.961	—1.853	—1.755	—1.667	—1.582
.2	—1.500	—1.425	—1.353	—1.283	—1.218	—1.155	—1.093	—1.036	— .980	— .925
.3	— .873	— .823	— .773	— .723	— .675	— .629	— .583	— .538	— .495	— .451
.4	— .408	— .366	— .324	— .283	— .242	— .201	— .161	— .120	— .080	— .040
.5	.000	.040	.080	.120	.161	.201	.242	.283	.324	.366
.6	.408	.451	.495	.538	.583	.629	.675	.723	.773	.823
.7	.873	.925	.980	1.036	1.093	1.155	1.218	1.283	1.353	1.425
.8	1.500	1.582	1.667	1.755	1.853	1.961	2.081	2.202	2.338	2.492
.9	2.667	2.867	3.100	3.373	3.713	4.128	4.694	5.497	6.857	9.899

Summary

We have developed a rational equation for the learning function which seems to agree with some of its known characteristics. On the basis of the present analysis we may formulate a series of hypotheses about rote learning, all of which are capable of experimental test.

1) A learning task that is homogeneous (elements of equal complexity) gives a symmetrical *S*-shaped learning curve which satisfies the rational equation.

$$\frac{2p-1}{\sqrt{p-p^2}} = \frac{kt}{\sqrt{m}} + z_0 \quad . \quad . \quad . \quad . \quad [27]$$

in which p = proportion of successful acts in unit time
t = practice time
k = learning constant of the subject. It is the average number of errors eliminated in each exposure.
m = complexity of the task
z_0 = value of z when $p = p_0$. It may be regarded as a constant.
p_0 = value of p when $t = 0$.

2) The learning curve for a homogeneous task has an inflection point at $p = .5$.

3) A learning task that is heterogeneous (elements of unequal complexity) gives a non-symmetrical curve with an inflection point in the lower half of the curve.

4) The more heterogeneous the task, the lower will be the inflection point. For very heterogeneous tasks the inflection point will be so low that the curve will show only negative acceleration, especially if experimental data for the early stage of learning are missing.

5) If a heterogeneous task is composed primarily of two levels of complexity, one part rather simple to learn while another part is more complex, then the simple part may be learned almost to perfection before the complex part is begun. Such a heterogeneous task will show a plateau. The plateau may also be found, although less pronounced, when two such tasks are started simultaneously.

6) When a plateau appears because of the combination of two tasks of different complexities, the rise of the first part will be steeper than that of the second part.

7) If a subject's learning constant k is known for a given type of learning material and if the complexity m of a certain task has

been determined in other experiments, then the subject's learning curve on this task can be predicted by the above equation for constant conditions of motivation.

8) If the learning constants for several subjects are known for a given type of learning material and if they all learn the same task, then the complexity m for this task should be the same in all of their learning curves.

9) The waning of interest on the part of the learner has the effect of moving the inflection point to the lower half of the curve.

10) If the subject improves his method during the course of learning a homogeneous task, the inflection point will move to the upper half of the curve.

11) If the subject fails to spread his attention equally over all of the items in a homogeneous task, then the inflection point will be in the lower half of the curve.

12) The phenomenon called insight appears when the task consists essentially in a very small number of psychological elements to be acquired or eliminated. These elements may be simple as in a coarse discrimination task or they may be the few conceptual points in a complex task. The former is a task with low value for the complexity m. The latter is a task which is mastered quickly by a subject with high learning constant k.

13) The relation between length of a homogeneous task and learning time is described approximately by the rational equation

$$T = \frac{c}{k} n\sqrt{n-a} \quad . \quad . \quad . \quad . \quad . \quad . \quad [40]$$

T = total learning time measured to any arbitrarily stipulated degree of perfection
n = length of the task
k = subject's learning constant
c = a constant depending on the complexity of the task and the degree of perfection required
a = adaptation constant of the subject. It is usually his attention span.

14) The relation between length of a homogeneous task and the number of repetitions required to learn it is described approximately by the following rational equation

$$R = \frac{c}{k} \sqrt{n-a}$$

in which R = number of repetitions required for complete learning.

15) If the individual learning curves of a group of subjects are symmetrical and *S*-shaped, then the composite or average learning curve for the whole group will have a low inflection point so that most of the composite curve will be negatively accelerated.

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L'APPRENTISSAGE

(Résumé)

On a développé une équation rationelle pour l'apprentissage laquelle semble concorder avec quelques-unes de ses caractéristiques connues. Sur la base de cette analyse on peut formuler une série d'hypothèses sur l'apprentissage par cœur, dont chacune peut être mise à l'épreuve expérimentalement.

1) Une tâche d'apprentissage laquelle est homogène (dont les éléments sont de la môme complexité) donne une courbe symétrique d'apprentissage, en forme S, laquelle satisfait l'équation rationnelle

$$\frac{2p-1}{p-p^2} = \frac{kt}{\sqrt{m}} + z_0,$$

où p = la proportion des actes réussis en temps d'unité, t = temps de pratique, k = constante d'apprentissage du sujet (le nombre moyen des erreurs éliminées dans chaque exposition), m = la complexité de la tâche, z_0 = la valeur de z quand $p = 0$ (on peut le considérer constant), p_0 = la valeur de p quand $t = 0$.

2) La courbe d'apprentissage pour une tâche homogène a un point d'inflexion à $p = 0{,}5$.

3) Une tâche d'apprentissage laquelle est hétérogène (dont les éléments ne sont pas de la même complexité) donne une courbe non symétrique au point d'inflexion dans la basse moitié de la courbe.

4) Plus la tâche est hétérogène, plus le point d'inflexion sera bas.

5) Si une tâche hétérogène se compose en grande partie de deux niveaux de complexité, une partie assez facile à apprendre tandis que l'autre est plus complexe, on peut apprendre presque parfaitement la partie facile avant de commencer à apprendre la partie complexe. Une telle tâche hétérogène montrera un plateau.

6) Quand il a lieu un plateau à cause de la combinaison de deux tâches de complexités différentes, l'élévation de la première partie sera plus perpendiculaire que celle de la seconde partie.

7) Si la constante k d'un sujet est connu pour un type donné de matière à apprendre et que la complexité m d'une certaine tâche ait été déterminée en d'autres expériences, la courbe d'apprentissage du sujet pour cette tâche peut être prédite par l'équation donnée en haut pour les conditions constantes des mobiles.

8) Si les constantes d'apprentissage pour plusieurs sujets sont connus pour un type donné de matière à apprendre et qu'ils apprennent tous la même tâche, la complexité m pour cette tâche devrait être la même dans

toutes leurs courbes d'apprentissage. (9) La diminution de l'intérêt de la personne qui apprend a l'effet de faire mouvoir le point d'inflexion à la basse moitié de la courbe. (10) Si le sujet rend meilleure sa méthode pendant l'apprentissage d'une tâche homogène, le point d'inflexion ira à la moitié supérieure de la courbe. (11) Si le sujet ne fait pas attention également à toutes les parties d'une tâche homogène, le point d'inflexion sera dans la partie inférieure de la courbe. (12) Le phénomène dit compréhension apparaît quand la tâche se compose essentiellement d'un très petit nombre d'éléments psychologiques à acquérir ou à éliminer. (13) La relation entre la longueur d'une tâche homogène et le temps d'apprentissage est décrite approximativement par l'équation rationnelle

$$T=\frac{c}{k}n\sqrt{n-a}$$

où T = la durée totale de l'apprentissage mesurée à n'importe quel degré arbitrairement stipulé de perfection, n = la longueur de la tâche, k = la constante d'apprentissage du sujet, c = une constante dépendant de la complexité de la tâche et du degré de perfection exigé, et a = la constante d'adaptation du sujet (ordinairement la portée de son attention). (14) La relation entre la longueur d'une tâche homogène et le nombre de répétitions exigées pour l'apprendre est décrite approximativement par la suivante équation rationnelle

$$R=\frac{c}{k}\sqrt{n-a}$$

où R = le nombre de répétitions nécessaires pour l'apprentissage complet. (15) Si les courbes individuelles d'apprentissage d'un groupe de sujets sont symétriques et en forme S, la courbe d'apprentissage composée ou moyenne pour tout le groupe aura un point d'inflexion peu élevé de sorte que la plupart de la courbe composée négativement accélérée.

THURSTONE

DIE LERNFUNKTION

(Referat)

Wir haben eine rationale Gleichung für die Lernfunktion enwickelt, die mit einigen ihrer bekannten Merkmalen übereinzustimmen scheint. Auf Grund der gegenwärtigen Analyse können wir eine Reihe von Hypothesen über das Auswendiglernen formulieren, die alle experimenteller Prufüng unterzogen werden können.

1) Eine zu lernende Aufgabe, die homogener Natur ist (Elemente von gleicher Verwickelung) ergibt eine symmetrische *S*—förmige Lernkurve, die der rationalen Gleichung

$$\frac{2p-1}{p-p}=\frac{kt}{\sqrt{m}}+z_0,$$

genügt, worin p die Proportion der in der Zeiteinheit vollzogenen erfolgreichen Akte ist, t die Übungszeit, k die Lernkonstante der Versuchsperson. (es ist die durchschnittliche Zahl der in jeder Exposition eliminierten Fehler), m die Verwickelung der Aufgabe, z_0 der Wert von z, wenn $p=p_0$ (mag als Konstante betrachtet werden), p_0 der Wert von p wenn $t=0$.

2) Die Lernkurve einer homogenen Aufgabe hat einen Beugungspunkt (Inflection point) bei $p=0{,}5$.

3) Eine heterogene Lernaufgabe (Elemente ungleicher Verwickelung)

gibt eine nicht-symmetrische Kurve mit einem Beugungspunkt in der unteren Hälfte der Kurve.

4) Je heterogener die Aufgabe, um so tiefer liegt der Beugungspunkt.

5) Wenn eine heterogene Aufgabe hauptsächlich aus zwei Stufen der Verwickelung besteht, ein Teil aus eher einfachen, der andere eher komplizierten Lernprozessen, dann kann der leichte Teil fast vollkommen gelernt werden, bevor der komplizierte begonnen wird. Eine solche heterogene Aufgabe ergibt ein Plateau.

6) Wenn durch die Kombination zweier Aufgaben von verschiedener Verwickelung ein Plateau entsteht, so ist der Aufstieg des ersten Teils steiler als der des zweiten.

7) Wenn die Lernkonstante k einer Versuchsperson für einen gegebenen Typus des Lernmaterials bekannt und die Verwickelung m einer gewissen Aufgabe in anderen Experimenten bestimmt worden ist, dann kann die Lernkurve der Versuchsperson an der gegebenen Aufgabe mittelst der obigen Gleichung vorausgesagt werden, unter der Bedingung, dass der Motivierungszunstand konstant bleibt.

8) Wenn die Lernkonstanten mehrerer Versuchspersonen bekannt sind für einen gegebenen Typus von Lernmaterial, und sie alle dieselbe Aufgabe lernen, dann muss die Verwickelung m für diese Aufgabe für all ihr Lernen dieselbe sein.

9) Nimmt das Interesse des Lernenden ab, so bewegt sich der Beugungspunkt in die untere Hälfte der Kurve hinunter.

10) Wenn die Versuchsperson ihre Methode während einer homogenen Lernaufgabe verbessert, bewegt sich der Beugungspunkt in die obere Hälfte der Kurve.

11) Wenn die Versuchsperson bei den einzelnen Posten einer homogenen Aufgabe in ihrer Aufmerksamkeit schwankt, dann ist der Beugungspunkt in der untern Hälfte der Kurve.

12) Die Erscheinung, die man als Einsicht bezeichnet, zeigt sich hauptsächlich wenn in einer Aufgabe sehr viele psychologische Elemente zu erwerben oder zu eliminieren sind.

13) Die Beziehung zwischen der Länge einer homogenen Aufgabe und der Lernzeit kann annähernd durch die rationale Gleichung

$$T = \frac{c}{k} n\sqrt{n - a}$$

dargestellt werden, worin T die gesammte zu einem willkürlich bestimmten Grade der Vollkommenheit gemessene Lernzeit bedeutet, n die Länge der Aufgabe, k die Lernkonstante der Versuchsperson, c eine von der Verwickelung der Aufgabe und dem Grad der verlangten Vervollkommnung abhängige Konstante, und a die Anpassungskonstante der Versuchsperson (gewöhnlich seine Aufmerksamkeitsspannung).

14) Die Beziehung zwischen der Länge einer homogenen Aufgabe und der zum Lernen notwendigen Anzahl der Wiederholungen ist annähernd durch die folgende rationale Gleichung auszudrücken

$$R = \frac{c}{k} \sqrt{n - a}$$

worin R die fürs vollständige Erlernen notwendige Anzahl der Wiederholungen ist.

15) Wenn die individuellen Lernkurven einer Gruppe von Versuchspersonen symmetrisch und S-förmig sind, dann hat die zusammengesetzte oder mittlere Lernkurve für die ganze Gruppe einen tiefen Beugungspunkt, sodass die meisten zusammengesetzten Kurven negative beschleunigt sein werden.

THURSTONE

AN INVESTIGATION OF BINOCULAR SUMMATION: I THE FOVEA*

From the Psychological Laboratories of Clark University

C. H. Graham

I

Since the time of Piper (8) the problem of binocular summation has received various forms of treatment. Piper found that in the dark-adapted eye there is a summation, so that sensitivity is about twice as great with both eyes as with one eye. Sherrington (11) also found some very slight evidence for summation, but Abney and Watson (1) got results which were definitely opposed to these.

It is the purpose of this paper to determine whether or not summation exists in the fovea. The method followed is somewhat similar to that of Piper for the periphery, but takes into consideration Hecht's criticisms (4, 5) of Piper's method and the methods of the older investigators.

II

At this point it is necessary to consider Piper's method in some detail. The method consisted essentially of determining thresholds for one eye and for two eyes after varying degrees of dark adaptation. The results for two eyes, when treated according to Piper's method, show that the sensitivity of the light-adapted retina, after a stay in the dark, grows at first slowly during the first 10 or 12 minutes, then more rapidly, to attain, after about 45 minutes, a relative maximum. When the course of adaptation for one eye was determined, the results showed that the binocular and monocular curves coincide for the first 15 minutes, but that after that period the binocular curve rises very sharply, while the slope of the monocular curve is increased very little. Thus, when the eyes are completely dark-adapted, the sensitivity of two eyes, as measured in terms of *Empfindlichkeit,* is about twice as great as that of one eye.

Hecht (4) has protested that the use of such vague measures as *Empfindlichkeit* not only obscures the true relation between the in-

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tensity of the stimulating light and the action in the retina, but also gives a wrong picture of the course of adaptation. The measure of *Empfindlichkeit*, as used by Piper, was defined as a million times the reciprocal of the minimum intensity on the assumption that the threshold is an inverse measure of sensitivity. Hecht maintains that we can know nothing of the "sensitivity" of the retina. We can merely state that after a short time in the dark the minimum intensity is large and that as the stay in the dark becomes longer and longer the minimum intensity becomes less and less until it reaches a constant value. When Piper's curves are corrected from this point of view, they become the typical curves of a bimolecular reaction as compared with their long S-shaped form when plotted in terms of *Empfindlichkeit*.

Another general criticism of Piper's method is that he made measurements at too frequent intervals. Not only is this true in the first section of his work where he determined the adaptation curve for two eyes (5), but it is also true in the second section dealing with summation. For example, in one case a binocular reading was made after 3½ minutes dark adaptation, a reading for the right eye after 4½ minutes, and a reading for the left eye after 5 minutes. Obviously, the time interval between determinations was of such a nature as to affect the form of the curve. The action of the liminal stimulus at one determination may have seriously altered the true threshold at a later determination. Piper mentions the fact that this possibility was controlled by making binocular and monocular determinations on different days. The results under these conditions were the same. However, when the general results of the experiment are so obscure, it is necessary to consider all the possibilities, since we have no way of knowing to what extent his conclusions were based upon faulty procedure.

Sherrington (11), in a problem on binocular flicker, found some evidence for summation. He demonstrated that with alteration of phases in the right and left retinas the number of phases per second just extinguishing flicker is slightly lower than with synchronism of similar phases in the right and left retinas. A set of his results is given here.

Subject	No. of phases per second just extinguishing flicker	
	Synchronism of similar phases in right and left retinas	Alternation of phases in right and left retinas
S. C. M. S.	97.3	95.8
B. M.	122.3	118.9
G. C.	123.0	119.8
G. C.	93.3	90.6
G. C.	73.4	71.8
C. S. S.	72.1	69.8

"From these observations it appears that similar phases of flickering illumination if timed to fall coincidentally on conjugate retinal areas do *very slightly reinforce* each other in sensation, and if timed exactly alternately do *very slightly mutually reduce.*" Here there may be some slight indication of summation, but it is noteworthy that the observations presented are single determinations. It is possible that a statistical analysis based upon many cases would have given different results. However, the definite trend of the results is indicative. The experiments were restricted almost wholly to the central region of the retina, and the intensity of illumination was above the threshold for the light-adapted eye. These results will be considered at greater length in Section VI.

Abney and Watson (1), working on the problem of color thresholds, determined that, with the two eyes together, at 5° from the fovea the threshold values agree with those corresponding to the better of the two eyes taken alone. At the fovea the threshold values are the same for either eye or for both eyes. The authors point out that their results may not be strictly comparable to those of Piper since the methods are not comparable.

In addition to these quantitative experiments, there are many qualitative reports of differences between monocular and binocular observations (9, 7, 14, 2). In these cases the illumination was relatively bright. Results from all these experiments tend to show that a shift from monocular to binocular observation results in an increase in brightness.

III

The apparatus used in this experiment was, with a few modifications, similar to that used by Hecht (5) in his work on foveal dark adaptation. As Hecht has pointed out, there are three major sources of error entering into the studies on dark adaptation. All of these are of significance for this problem. (*a*) Piper's method of securing an original condition of light adaptation was most haphazard. A stay in the open air (from 15 to 60 minutes), during which the subject regarded objects illuminated by a medium intensity, was considered an adequate period for light adaptation. Little thought was paid to the variables of intensity of illumination or time. (*b*) The break from light adaptation to dark adaptation was not clean and quick. The nature of the light-adapting method was such that the subject found it necessary to walk or run from outside into the dark

room. In this interval no measurements could be made. (*c*) Pupil changes were not controlled. This factor in the present problem will be discussed at length in Section VI.

The apparatus used here eliminated the first two sources of error. Observations were all in a dark room. Light adaptation was secured by having the subject look from a uniform distance at a brightly illuminated surface for a definite interval. Dark adaptation in this

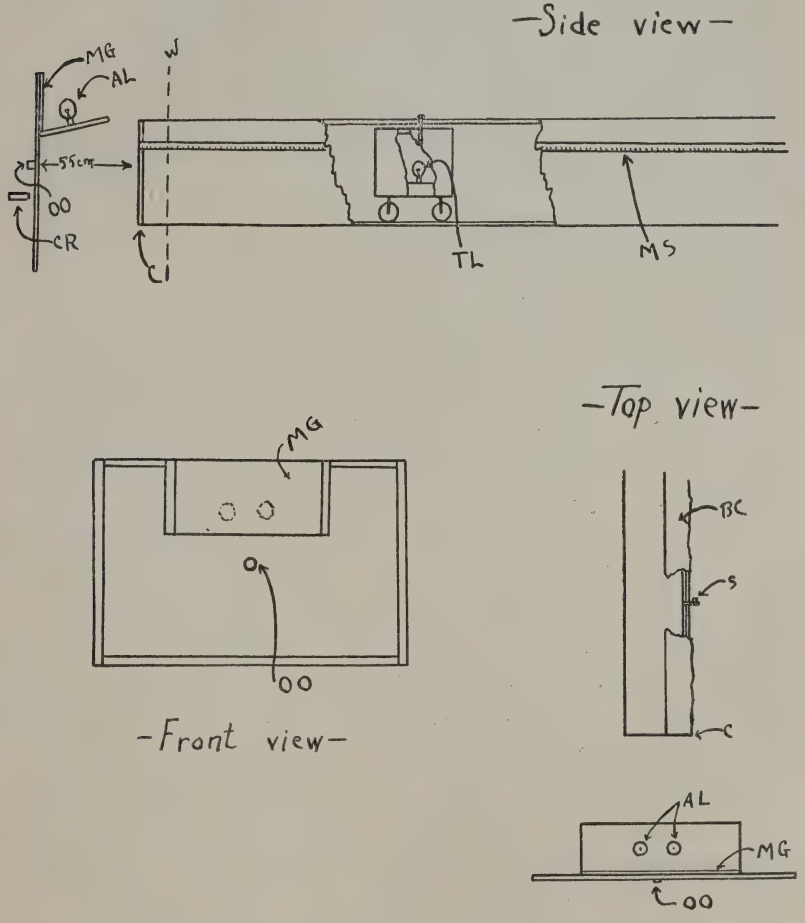

FIGURE 1
THREE VIEWS OF THE APPARATUS USED IN THIS EXPERIMENT (Explanations in text)

procedure was brought about by switching off the screen (surface) illumination. After light adaptation the subject moved his head about 15 cm. and was ready for the measurement. The threshold-determining object was a small red cross. This was used in order to insure foveal vision, the characteristic perception of the fovea being of form and color. The area of the cross was such that its image fell well within the macula.

The drawing of the apparatus is presented in Figure 1 which shows three views of the arrangement. The light-adapting screen is marked *MG*. It is a large plate of milk glass, 60 by 30 cm. Behind this at a distance of 18 cm. are two 250-watt, 110-volt, concentrated-filament Mazda lamps. These lamps are turned on or off by a switch controlled by the experimenter. The subject fixates the area of the screen between the two lights. In the diagram this is represented by the area between the broken circles in the front view. The brightness of this area was 130 millilamberts. This part of the apparatus is in the dark room.

The apparatus for measuring the thresholds is also presented in Figure 1. This is arranged so that all the adjustments can be made by the experimenter who is not in the dark room. The set-up consists of a blackened box, 400 cm. long, 24 cm. high, and 18 cm. wide. This box extends through an opening in the dark-room wall, about 370 cm. of the total length being outside the dark room. The small crack between the box and the wall is well sealed so that no light may enter the dark room. This arrangement is shown in the side view, *W* showing the position of the wall. At *C* there is a plate of milk glass. Placed against this, and on the side facing the observer, is a Wratten Filter No. 70. This excludes all rays below 650 mμ and restricts vision to the cones. In front of the filter is a cross-shaped opening in an opaque piece of cardboard. The cross is 24 mm. in diameter, with an area of 320 sq. mm. The subject's eye is placed at the observation opening, *00*, which is a round hole with a shield about it extending about 1.5 cm. towards the subject. When binocular determinations are made there are two of these openings, so adjusted as to allow for easy focusing of the eyes. The distance from the eye to the cross is 55 cm. The cross gives, under these conditions, an image of 0.65 mm. in diameter which falls within the cone area of the eye.

The red cross is illuminated by means of a 25-watt, 110-volt, concentrated-filament Mazda lamp (*TL*). The lamp is in position

in a movable carriage which rides freely in the long, blackened box. The carriage is made to fit the box so that there is very little "play." The light from this lamp is diffused by the milk-glass plate and then passes through the filter to the eye. Variations in intensity of illumination of the red cross are brought about by moving the lamp (*TL*) away from or closer to the milk-glass plate. The intensity of the red cross is measured at a given position and the intensity at any other position is determined by the inverse square law. The lamp was always further than 55 cm. from the red cross.

The wires for the lamp *TL* run up the handle of the movable carriage. On the handle is a switch (*S*) controlled by the experimenter. The slit through which the handle extends is covered in its whole length with a black cloth, *BC*. In addition to the handle, a pointer is attached to the carriage which indicates the positions of the light on the meter stick, *MS*.

The timing of the dark-adaptation process is done by means of a stop-watch. The interval between the turning on of the threshold light and the subject's reaction is measured by a timing device consisting of a Jacquet clock so arranged that it can be stopped or started by activation of small electro-magnets. Synchronously with the turning on of the threshold light the experimenter throws a switch which starts the Jacquet clock. Conveniently situated with respect to the subject is a key which he presses when he sees the red cross. Pressure on this key closes a circuit through the other electro-magnet, which stops the clock.

IV

Nine subjects were used in this experiment. All but one, a medical student, were connected with the department of psychology. With, seven of the subjects five complete series of determinations were made. With two, only one binocular and one monocular determination were made. In the case of the group of seven the first series of determinations was used as a preliminary practice period.

The subject was allowed 3 minutes of dark adaptation before the light-adapting lights (*AL*) were turned on. This allows a close approximation to a constant starting-point for all subjects. The subject was seated comfortably in a chair which was placed in a fixed position. Under these conditions the eyes of the subject were on a level with the observation openings and about 35 cm. distant from the light-adapting area of the milk-glass plate (*MG*) which

was slightly above them. Five minutes of light adaptation were allowed. Near the end of this period a signal was given by the experimenter and at the proper time the lights were turned out. A movement of about 15 cm. was enough to bring the eyes of the subject to the observation opening. A chin rest (*CR*) was provided.

The method of making monocular determinations was identical with that used by Hecht. Before each determination the subject was instructed which eye was to be used. Before the bright lights were turned off the threshold light was placed at such a distance in the long box that the subject would see it after about five seconds. When the adapting lights were turned off, the subject looked in and pressed on the key when he saw the red cross. As soon as the key was pressed, the experimenter turned off the threshold light and pushed the carriage to the other end of the long box. Time and distance readings were noted.

Two minutes after dark adaptation had started a determination was made with the right eye. The threshold lamp was turned on and pushed nearer and nearer to the cross at a constant rate. The subject again pressed on the key when he saw the cross. The time was read on the Jacquet clock which was started exactly 2 minutes after dark adaptation had begun. Time and distance readings were again taken. This process was again repeated after 6 minutes dark adaptation with the left eye. Determinations were made at 10 minutes with the right eye, 15 minutes with the left eye, and 20 minutes with the right eye.

After the last observation the bright adapting lights were turned on and the subject was light adapted for 5 minutes. The entire process described in the last paragraph was repeated, except that the first observation was made with the right eye, the others following alternately. Thus a set of determinations was secured for each eye, covering the period from the beginning to the end of dark adaptation. Intermediate determinations were made for each eye after 30 seconds and after 3 minutes dark adaptation, and after 15 seconds and after 1 minute dark adaptation. In these cases there was a 5-minute light-adaptation period as before. The order of these later determinations was as follows: light adaptation, left eye after 30 seconds, right eye after 3 minutes. This was then reversed. The other determinations had the order: light adaptation, left eye after 15 seconds, right eye after 1 minute. This, too, was then reversed.

The procedure for two eyes was similar except that the observa-

tions had to be made consecutively. The order for these series of determinations was as follows: light adaptation, both eyes after (about) 5 seconds, 5 minutes, 10 minutes, 15 minutes, and 20 minutes. Intermediate determinations were made, each requiring a light-adaptation period, at 15 seconds, 30 seconds, 1 minute, 2 minutes, and 3 minutes. In each observation period two complete series of determinations were made. It is to be noted that all measures of the threshold for two eyes have a corresponding measure for each eye, except the measure after 5 minutes, which corresponds to the monocular measure for 6 minutes. The objection might be raised that the determinations for two eyes were made at too rapid intervals. This is not a very tenable objection when it is shown that the course of foveal dark adaptation is almost complete in 30 seconds. The ideal way would be to make only one determination after each light adaptation, but the effects on the subject would be so pronounced that a compromise is necessary.

TABLE 1
RECORD OF A SINGLE SERIES OF MONOCULAR DETERMINATIONS

Left eye			Right eye		
Order	Duration	Distance	Order	Duration	Distance
1	:10	60	2	2:03	120
3	6:15	110	4	10:10	130
5	15:08	121	6	20:12	125
8	2:07	120	7	:10	60
10	10:02	133	9	6:10	115
12	20:02	170	11	15:05	135
13	:35	95	14	3:06	120
16	3:05	120	15	:31	92
17	:18	70	18	1:02	120
20	1:03	130	19	:20	67

Table 1 gives a concise picture of the procedure. The table shows, first, the order of observations; secondly, the period from the time the light-adapting lights were turned off to the time when the subject pressed the key announcing that he had seen the cross; and, thirdly, the distance at which the threshold lamp was set when the cross was seen. Each space between groups indicates 5 minutes light adaptation.

V

The nature of the data secured in this experiment is illustrated by Figure 2. This is a graph of the results obtained with two subjects

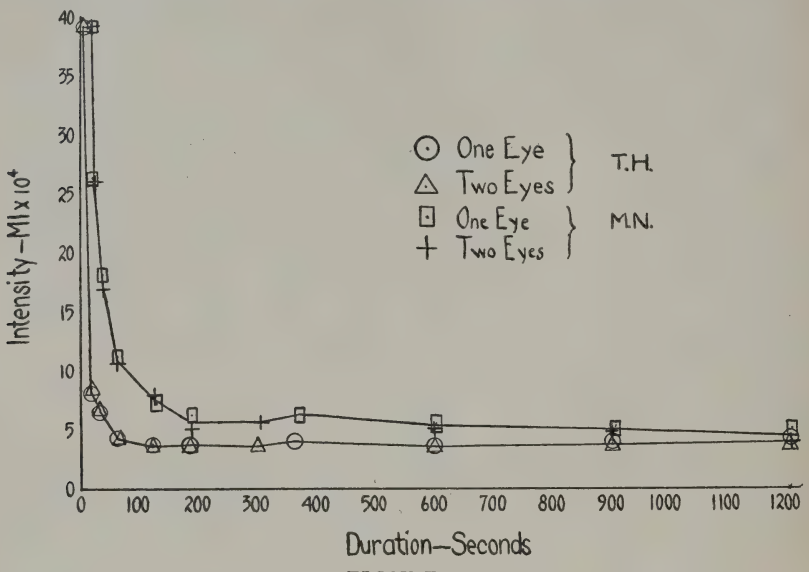

FIGURE 2

BINOCULAR AND MONOCULAR THRESHOLDS FOR THE TWO SUBJECTS SHOWING THE GREATEST DEVIATIONS FROM THE AVERAGE

in eight observation periods. Each point is the average of four determinations. In the case of the monocular determinations there are two for each eye. The data presented in this figure show the range of individual variations, particularly for the earlier determinations. The results given are taken from those individuals showing the greatest variation from the average. It is to be noticed that, although there are rather marked individual differences, the general shape of the curve is consistent for each individual. The curve of T. H., that curve showing the most precipitous course of dark adaptation in the first few seconds, has extremely low values for the first few determinations. M. N. shows values which are quite a little above the average. Disregarding these points of difference, one notices at once the striking uniformity of the results.

It might look as though in the case of M. N. there is a slight summation effect. This must be discounted when it is shown that

other observers showed the reverse tendency. In these cases the threshold for one eye is lower than the threshold for two eyes. All variations showing a summation effect fall well within the limits of the experimental error. The values from which the points on the curves are plotted are given in Table 2.

TABLE 2
MONOCULAR AND BINOCULAR THRESHOLDS FOR TWO SUBJECTS

Subject							
T. H.				M. N.			
One eye		Two eyes		One eye		Two eyes	
T*	I †(mlx10⁴)	T	I (mlx10⁴)	T	I (mlx10⁴)	T	I (mlx10⁴)
4.00	39.1	3.50	39.1	17.00	39.1	15.00	39.1
16.75	8.2	17.00	8.7	20.25	26.7	24.10	24.3
33.00	6.4	34.00	6.7	36.40	18.0	33.25	17.0
62.75	4.5	63.50	4.1	62.00	11.4	68.00	10.3
123.25	3.7	123.33	3.6	127.30	7.1	127.30	8.0
183.00	3.7	183.00	3.7	187.00	6.0	185.25	4.9
363.50	3.9	303.33	3.4	368.25	6.1	304.00	5.5
604.00	3.6	603.60	3.4	605.00	5.6	603.25	4.9
904.50	3.9	903.50	3.2	906.75	5.1	901.75	4.4
1205.00	4.1	1202.50	3.4	1205.00	4.7	1201.00	3.7

*T = Time of dark adaptation in seconds.
† = Intensity of threshold light.

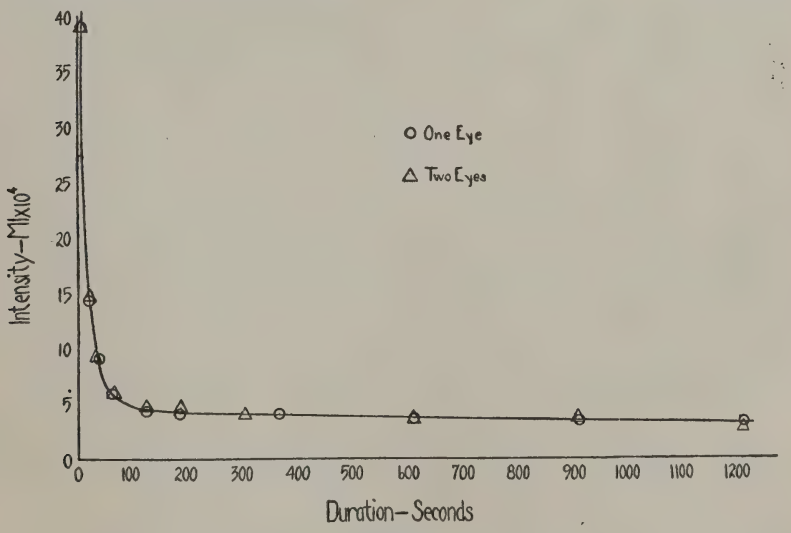

FIGURE 3
AVERAGE BINOCULAR AND MONOCULAR THRESHOLDS FOR NINE SUBJECTS (Each point is the average of at least 30 determinations)

The complete data are presented in Table 3. The tabulations entered here are the combined measurements of the nine subjects. The graphical presentation is given in Figure 3. The agreement with the individual curves in Figure 2 is striking. The data presented in this composite manner have the advantage of presenting the theoretical process. Because of this averaging of the results of a number of subjects it is not necessary to draw conclusions as to the binocular *vs.* the monocular effect from the results of separate observers. If this were the case one might say that some subjects showed a summation and others did not. As was pointed out in discussing the curve of M. N., any summation effect shown is well within the error of determination. The composite data show that the monocular thresholds are consistently as low as the binocular.

There is one other fact evident in the data which must be presented. That concerns the process of dark adaptation as shown by either binocular or monocular observation. Hecht (5) in writing of his results says: "The threshold after 7 seconds of dark adaptation is about 40 x 10^{-4} ml. After 30 seconds in the dark the threshold drops to about 12 x 10^{-4} ml.....During the next 20 minutes the threshold further decreases, but only to about 3 x 10^{-4} ml. and remains there." The data of this experiment are not strictly comparable with Hecht's since the adapting lights had a brightness of 130 millilamberts as opposed to Hecht's brightness of 90 millilamberts, but the general agreement of the results is apparent.[1]

VI

The results which have been presented show that within the limits

[1]It would be of interest to know the threshold of the completely dark-adapted fovea expressed in radiometric terms. The translation from the photometric measures can be made by means of Troland's analysis (On the measurement of visual stimulation intensities. *J. Exper. Psychol.*, 1917, **2**, 3-16). The radiometric values can be determined by means of the equation $w' = .00132 \times 10^7 b / v_\lambda$, where w' is *ergs per second per steradian per square meter*; .00132 is the mechanical equivalent of light; 10^7 is the constant for converting watts into ergs per second; b is the photometric brightness (candles per square meter); v_λ is the visibility coefficient for the specified wave length. If 700 $m\mu$ is accepted as the dominant wave length of the filter, v_λ is .00241 (average of Nutting's and Hyde and Forsythe's values as given in Troland). If the final dark-adaptation threshold is taken to be 3.5×10^{-4} ml., the radiometric value is given by $w' = 61 \times 10^3$.

TABLE 3
AVERAGE MONOCULAR AND BINOCULAR THRESHOLDS FOR NINE SUBJECTS

One eye		Two eyes	
T*	I† (mlx10^4)	T	I (mlx10^4)
7.5	39.1	7.5	39.1
18.6	14.4	18.5	15.0
35.6	8.7	34.8	9.2
65.1	6.0	67.0	5.9
125.4	4.5	125.0	5.0
186.0	4.2	189.0	4.9
366.0	4.1	305.6	4.1
604.4	3.7	605.6	4.1
905.0	3.5	904.0	3.9
1205.0	3.7	1204.0	3.5

T* = Time of dark adaptation in seconds.
† = Intensity of threshold light.

of the sensitivity of the method there is no summation effect. However, there is one variable which has not been controlled, i.e., pupil area.

Sherrington (11) was the only one of the earlier workers who took notice of this factor. Piper (8) and Watson and Abney (1) derived results for the eye as a whole. After a consideration of the literature one is forced to wonder as to the possible effects of pupil area in summation experiments. Weiler (13) found some slight evidence to show that there is greater contraction of the pupil when the two eyes are directly stimulated than when one eye alone is stimulated. He also showed that, after the pupil areas of both eyes have reached a consensual maximum contraction (produced by a known amount of light on one eye), if the same amount of light is allowed to fall on the other eye, a secondary contraction results and the pupil areas become smaller than the first minimum.

Such a phenomenon might be considered in discussing Sherrington's results. It is true that he used an artificial pupil, but the artificial pupils used were 3 mm. or more in diameter, whereas the minimum pupil diameter for lights of medium intensities is between 2 and 3 mm. (10). It can be seen that even with the employment of a pupil slight variations may have resulted. It is to be remembered that he found the threshold for flicker to be higher with *alternate* phases. The iris behavior resulting may have been that type which Weiler reports. Of course the phases of flicker were so rapid that one phase could not have made much difference, but it is possible that over a period of phases some such phenomenon might have occurred. With

synchronous phases there could not have been any opportunity for the secondary effect, and so contraction would not have been so great. If this were the fact, the amount of light striking the retina during the synchronous phases would have been more than during the alternate phases. Of course the differences may not have been great, but it is conceivable that it might have been great enough to produce the slight differences in flicker thresholds reported by Sherrington.

Another possibility as to the influence of the iris is made evident by the work of Reeves (10). Reeves found that over a large range of light intensities the pupil diameter is greater in one eye when the other eye is closed than when both eyes are open together. Under the conditions of the experiments reported here light adaptation was always with two eyes, so that the final level for dilation whether for one eye or for two should have been constant. There is, however, a slight chance that the threshold light caused a noticeable contraction. Under these conditions the area for one eye would have been greater than for two eyes, hence the monocular threshold would have been lower than the binocular. These results would be opposed to any evidence for summation.

The results reported here show no evidence for summation. If the threshold light had any effect it would have been to cause a lower threshold for one eye. The question then arises as to whether or not a real summation effect was the factor which produced comparable thresholds for one eye and for two eyes—i.e., did the pupil effect just compensate a summation effect, This question would arise on the basis of Reeves' results. It should not be forgotten, however, that Reeves' results are from a comparison of the pupil areas of two eyes and the pupil area of one eye with the other eye closed. One cannot say whether the differences are due to the light reaction or to some type of reaction between the muscles controlling the eyelid and the iris in the other eye. This question would arise if by "closed" eye Reeves means, not a covered eye, but an eye with the eyelid closed.

This brief summary shows the necessity for control of pupil area. The amount of light striking each eye in a binocular observation must be the same as the light striking one eye during monocular observation. In order to test for this factor two subjects were used in a series of experiments. The apparatus was the same as in the first series, except that a very small fixation light was placed 3 cm. above

the red cross, and artificial pupils were used. The fixation light was no larger than a pinpoint. This was used to facilitate localization which was difficult with the artificial pupils. The artificial pupils had a diameter of 2 mm. One subject was tested for the threshold after 3 minutes and after 10 minutes in the dark. The other was tested after 2 minutes and after 7 minutes. It was considered that the dark-adaptation process had reached the final level in this time.

The fixation light was under the control of the experimenter. Twenty seconds before a determination the experimenter warned the observer and pressed the key controlling the fixation light. The light was never on for more than three seconds, or just long enough for the subject to adjust himself so that at the determined time the red cross would be within foveal fixation. Under these conditions the subject was allowed 17 seconds in the dark to compensate for the slight light adaptation produced by the fixation light.

The use of the artificial pupils had another desirable feature. By their use the apparatus became more sensitive; that is, greater intensities of light were necessary for stimulation and so differences became more noticeable.

Each subject was given at least two practice periods in observation before the final tests were made. With each subject 24 determinations were made: 4 with each eye for each dark-adaptation time and 4 with both eyes for each corresponding adaptation time. With one subject a series was run for one eye, using the conditions for two eyes, i.e., a determination was made at two minutes and a determination at seven minutes in the same eye to see if previous stimulation caused a difference in the final threshold. None was found.

The results are given in Table 4. It is to be noticed that in the case of C there is a difference in thresholds between the eyes, the right eye being more sensitive. The average deviations are fairly large, but the results are extremely consistent. Difficulty in localization was undoubtedly a great factor. G shows no great difference between the eyes. In all cases the final thresholds for two eyes are no lower than they are for the more sensitive eye. These results are in harmony with those of Abney and Watson (1) at 5° from the fovea. The differences at the fovea which Abney and Watson did not find are in this case made obvious, unquestionably because of the fact mentioned before, that the apparatus with artificial pupils is a more delicate indicator.

A comparison of the thresholds in this later series with the

TABLE 4

THRESHOLD VALUES FOR TWO SUBJECTS WITH ARTIFICIAL PUPILS

	Left eye		Right eye		Both eyes	
Subject C						
Time	186	606	183	603	185	603
I ($m1x10^4$)	38.1	36.7	27.1	23.4	28.1	29.3
A.D.	6.6	5.6	4.2	3.0	2.5	5.8
Subject G						
Time	128	432	131	427	130	428
I ($m1x10^4$)	33.9	29.8	31.7	26.6	26.7	27.0
A.D.	3.0	5.1	8.4	6.6	6.0	3.4

thresholds of the earlier series is made possible by converting the data into photons, (12, 6). If we accept C's average threshold as 30×10^{-4} ml. and multiply it by a pupil area of 3.1416 sq. mm., corresponding to the area of the artificial pupil, we get a value of 95×10^{-4} photons. Accepting a pupil area of 40 sq. mm. as an average for the area of the natural pupil in the first series and multiplying it by a threshold value of 3×10^{-4} ml., we get a value of 120 photons. This is roughly comparable to the results with the artificial pupil, especially when it is remembered that the artificial pupil is not in the plane of the nodal point of the eye. The low value, 3×10^{-4} ml., was taken because G was a subject of the type showing a lower-than-average threshold. It is true that, although the photon values for the two series are roughly comparable, the difference may be too large to be overlooked. Such a difference might be explained on the basis of Geldard's results (3). He found that stimulation of one part of the retina made the adjacent area more sensitive to later stimulation. It is possible that the very small amount of light coming from the fixation light (which was never directly fixated and which served only as a point of reference for localization) may have caused an increased sensitivity in the fovea for the following threshold determination. However, this factor was always constant and in no way invalidates the later results. The slight difference in photon values might as well be attributed to differences in the amount of practice for the two series, or to the fact that the basis of determination is rough.

The results in this section show that the conclusions in the first series of experiments are justified. There is no summation effect in the fovea. These later series show more refined results in that

they determine the fact that the threshold for two eyes is the same as that of the more sensitive eye.

Summary

A review of the more important literature on the problem of binocular summation has been presented. By following a method which has been described it was determined that there is no summation effect in the fovea. The rôle of pupil area in a problem of this type has been discussed. It has been shown that when pupil area is controlled there is no summation, and further, that the thresholds for two eyes are approximately those of the more sensitive eye.

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UNE ENQUÊTE DE LA SOMMATION BINOCULAIRE I. LE FOVÉA

(Résumé)

Suivant une méthode décrite par Hecht, l'auteur a déterminé le cours de l'adaptation fovéale à l'obscurité dans des conditions d'observation monoculaire et binoculaire. Puisque le cours du processus est le même dans les deux conditions on conclut qu'il n'y a nulle sommation binoculaire dans le fovea. On présente ici un résumé critique de quelques-unes des contributions les plus importantes à ce sujet. Le rôle de l'aire de la

pupille dans un problème de ce type est discuté. On montre qu'il n'y a nulle sommation quand l'aire de la pupille est contrôlée (par l'usage de pupilles artificielles) et en outre, que les seuils pour les deux yeux sont approximativement ceux de l'oeil plus sensitif.

GRAHAM

EINE UNTERSUCHUNG BINOCULARER SUMMIERUNG. I. DIE FOVEA

(Referat)

Der Verfasser bestimmte den Verlauf der fovealen Dunkeladaption unter des monokularen und binokularen Sehens. Da der Verlauf des Prozesses unter beiden Bedingungen derselbe ist, folgt, dass es keine binokulare Summierung in der Fovea gibt. Die Arbeit enthält einen kritischen Überblick über die wichtigeren Beiträge zu diesem Gegenstand. Die Rolle des Pupillenfeldes in einem Problem dieser Art wird diskutiert. Es wird gezeigt, dass wenn das Pupillenfeld unter Kontrolle steht (durch Verwendung künstlicher Pupillen), es keine Summierung gibt, und ferner, dass die Schwellen für swei Augen ungefähr gleich sind denen des empfindlicheren Auges.

GRAHAM

PIANOFORTE DAMPER-PEDALLING UNDER TEN DIFFERENT EXPERIMENTAL CONDITIONS*[1]

From the Psychological Laboratory of The Johns Hopkins University

CHRISTIAN PAUL HEINLEIN

In a previous paper (2), the writer demonstrated experimentally that both professional and student pianists differ considerably in their pedal interpretations of a given musical composition, and that such individual differences in pedal interpretation are related in some measure to differences in the intensity of tonal shadings, in the speed of rendition, and in the various types of phrasing effected. Furthermore, it has also been demonstrated that, under essentially the same experimental conditions, well-trained pianists possessing a high degree of general intelligence as well as musical intellect find it exceedingly difficult, if not impossible, to achieve exact repetition of an initial pedal interpretation as an integral part of a given musical composition.

The present related investigation, in which the same four pianists were enlisted who had served as subjects in the previous experimentation mentioned above, was undertaken for the purpose of ascertaining the nature and degree of differences, if any, in damper-pedal performance of a given musical composition when the subject is required to pedal under ten different conditions representing ten different tasks.

The composition chosen for pedal interpretation was an excerpt from Schumann's familiar *Träumerei* (see Figure 1). No pedal marks nor suggestions were indicated on the written score. The composition was performed by each subject on a concert-grand Weber Duo-Art[2] reproducing piano, the damper mechanism of which was connected with the recording pen of an extensor kymograph.

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[1]The investigation recorded in this paper was carried on during the tenure of a National Research Fellowship in the Biological Sciences.

[2]Much of this experimentation was made possible through the kind co-operation of Mr. Franklin Dunham and through the generous loan of two Weber Duo-Art reproducing pianos by the Aeolian Company of New York City.

This kymographic method of recording pedal frequency, pedal incidence, and pedal amplitude has been described in detail by the writer elsewhere (2). In thus obtaining kymograms of each subjects pedal performance, the pedal frequency was related to temporal value, melodic incidence, and phrase characterization. Each subject attended several testing periods on different days. On the first day of experimentation, each subject performed four tasks in the order outlined subsequently. On the second day, Tasks 5 and 6 were undertaken. The remaining tasks were completed in the following order: Tasks 7 and 8 on the third day, Task 9 on the fourth day, and Task 10 on the fifth day. On days when more than a single task was undertaken, a 10-minute interval of relaxation was afforded each subject between successive tasks, during which period certain introspective reports were obtained and necessary kymographic adjustments effected.

Before any experimentation was begun, each subject was interviewed and directed to practice at home the particular excerpt from *Träumerei* until he was able to perform the composition from memory without any error. In order to satisfy the director that such memorization had been attained, before any records were taken each subject was required on the first day of experimentation to play first from the printed score and immediately thereafter from memory. In every instance, from simple attention to each rendition as a whole, each subject's rendition of recall seemed to the experimenter to coincide closely in its several characteristics with the initial sight-reading interpretation. The first experimentally recorded task shortly followed this preliminary testing.

In the first task, the subject was presented with the printed score and requested to give his own customary interpretation of the composition. In terms of damper-pedal frequency for each of the four subjects, the results of this performance, as well as those of succeeding performances, are presented in Table 1. As might be expected from the nature of the data obtained from the writer's previous investigation on damper-pedalling, every subject in this instance gave a different pedal interpretation. However, from the standpoint of pattern-type, each subject evidenced a somewhat strong inclination towards melodic correlation in pedal depression. Subject A occasionally introduced half-release, Subject B introduced frequent half-depression at the close of the first phrase, while Subject C employed neither half-release nor half-depression. For

TABLE 1

Damper-pedal frequency for 9 successive measures of Schumann's *Traumerei* in 10 different performances under 10 different conditions. The time required to play each passage of 9 measures is indicated in seconds in the column following the performance number. An exponent indicates half-release; a sub-numeral indicates half-depression.

In Performance 1 the subject plays and pedals from the printed score (see Figure 1); in Performance 2 the subject plays and pedals the same passage from memory; in Performance 3 the subject pedals while tapping from the printed score the melody on an electric key without tonal effects; in Performance 4 the subject pedals while tapping from memory the melody on an electric key without tonal effects; in Performance 5, without touching the keyboard, the subject pedals from the printed score while listening to Godowsky's rendition of the same passage as recorded on the Duo-Art piano; in Performance 6, without touching the keyboard, the subject pedals from memory while listening to Godowsky's rendition as recorded on the Duo-Art piano; in Performance 7, without touching the finger-board, the subject pedals from memory while singing or whistling the melody of the composition; in Performance 8 the subject pedals while playing with the right index finger the melody from memory; in Performance 9, without touching the keyboard, the subject, with eyes closed, pedals while imaging the composition as if being played by himself; and in Performance 10, without touching the keyboard, the subject pedals from memory while listening to a rendition of the same musical passage as recorded on the Panatrope by the Cleveland Symphony Orchestra.

SUBJECT A

Performance	Time in seconds	Measures 1	2	3	4	5	6	7	8	9	Total
		Damper-pedal Frequency									
1	50.4	1	2^1	3^1	5	4	3	3^1	6^1	2	29
2	57.7	0	2	3	5^3	3_1	1	3	6^1	2	26
3	55.5	0	1	1	2	2	1	1	2	3	13
4	52.6	0	1	1	1	1	0	1	2	1	8
5	48.8	0	1	1	2	2	1	1	4	4	16
6	49	0	1	1	2	2	1	1	2	4	14
7	51.2	0	1	1	2	2	1	1	2	3	13
8	51.8	0	3^1	1	2_2	2	2^1	1_1	4_1	3	22
9	52.7	.	.	.	.	.	.	.	.	.	14
10	55	0	1	1	2	2	1	1	2	2	12
		SUBJECT B									
		Damper-pedal Frequency									
1	45.6	1	1	2	3_2	3_3	2	1	5	5	28
2	49.3	1	2	1	5	8	2	1	7	4	31
3	53.9	0	1	1	4	7	1	1	6	4	25
4	54.5	0	2	4	6	8	3	4	6	4	37
5	48.6	2	1	1	5	6	2	2	6	5	30
6	48.9	1	1	3	6	3	3	3	7	5	32
7	47.3	0	3	3	6	2	2	4	6	4	30
8	55	1	3	3	6	7	3	3	6	4	36
9	49.3	.	.	.	.	.	.	.	.	.	32
10	—	(Subject not available for this performance)									

TABLE 1 (*continued*)

Performance	Time in seconds	Measures 1	2	3	4	5	6	7	8	9	Total
					SUBJECT C						
					Damper-pedal Frequency						
1	40.5	1	2	2	5	3	3	2	5	4	27
2	43.8	1	3	2	4	4	2	3	5	4	28
3	55.6	1	2	3	3	3	2	3	5	3	25
4	55	1	2	2	2	2	2	3	3	3	20
5	49	0	1	1	3	3	1	2	4	3	18
6	49.1	0	1	2	2	1	1	2	3	3	15
7	53.8	1	2	2	3	3	2	2	3	2	20
8	55.5	1	4	5	8_1	4	4	6	6	5	44
9	68.8	.	.	.	.	.	.	.	.	.	20
10	55	0	2	1	3	1	2	1	3	3	16
					SUBJECT D						
					Damper-pedal Frequency						
1	54.3	0	2^1	1	4	4	3	2	5	4	25
2	57	0	1	1	2	2	2	2	2_1	3	16
3	63.7	0	1	1	2	2	1	1	2	3	13
4	53.7	0	1	2	2	1	1_1	1	2_1	2	14
5	48.7	0	1	1	1	3	0	1	2	3	12
6	48.9	0	1	1	2_1	2_1	1	2	1_3	3_1	19
7	51	0	1	2	2	1	1	1	3	3	14
8	51.6	0	1	1	2_1	2^1	2	2	3^1	3	17
9	60.2	.	.	.	.	.	.	.	.	.	14
10	55	0	2	2	1	3	1	1	2	2	14

the most part, Subject D, like Subject C, employed total depression and release.

In the second task, each subject was asked to give from memory an exact replica in every detail of his first rendition which he had just completed. In comparing and relating each subject's pedal pattern of the first performance with his pedal pattern of the second performance, considerable variation in pattern distribution and in pattern type was found to exist. In regard to temporal aspects, for the total rendition every subject allowed more time in playing from memory than in playing from the printed page. This difference in time is accounted for principally in the last two measures of the composition in which the closing chords were sustained for a longer period.

In order to afford the reader a clear understanding of the nature of the differences in pattern type and in pattern distribution—differences which are not revealed in their fullest extent by any simple

statistical tabulation, however valuable such a tabulation may be for indicating depression frequencies—photostatic reproductions of the pedal kymograms of Subject A are inserted for illustrative purposes. In each kymogram, the top line is the time line, the line immediately below is the melodic line, the line below the melodic measure is the pedal pattern, while the bottom line is the phrase index. In view of the nature of the tasks which followed the second performance, it was impossible to obtain any phrase indications in these latter series.

Since these several pianists are college men who have had psychological training, their introspections are held to be highly significant and reliable. In reviewing their reports, every subject, with the exception of Subject C, expressed doubt in the duplicature of his response. Subject C felt confident that he had re-enacted his initial performance. A careful examination of his kymograms revealed that, despite the prevalence of some marked variations, he was able to achieve closer response approximation than were the other subjects. In performing from memory, Subject A introduced half-release in the fourth measure which was not a characteristic of his previous rendition, while Subject B, having initially introduced half-depression in the fourth and fifth measures, employed whole depression throughout repetition. In the second performance, Subject D shifted to harmonic pedalling, after having strongly inclined to melodic pedalling in the first performance. In his first performance, he depressed the pedal a total of 25 times, while in his second rendition, he depressed the pedal only 16 times.

Every subject declared that performing from memory represented quite a different musical experience than performing from the printed page. The change in direction which attention assumes was declared to be one of the several factors which modified response. In reading from the printed page, the experimenter noted that the subjects rarely looked down on the keyboard to observe either finger position or finger movement. In performing from memory, however, each subject directed his attention without exception to keyboard position and carefully observed his own finger movements. In playing from the printed page, the experience was related in which there was a tendency to ascribe to each note its exact time value, whereas in performing from memory, a realization of a certain freedom from such mathematically precise

temporal limitations coupled with greater emotional spontaneity in expression and a consciousness of projecting individuality into interpretation, led to greater durational variations and to differences in pedal pattern.

The type of imagery employed in recall seems to have a significant bearing upon the nature of pedal response. The imagery of Subject A was predominantly auditory. He heard anticipatorily not only the melodic figure but also the harmonic structure upon which the melodic figure was superposed. Such auditory anticipation frequently was intrumental in shifting pedal incidence, so that in some measures his pedal pattern assumed marked syncopated characteristics. He did not visualize the printed page, nor did he attend directly to any of his pedal movements (compare Figures 2
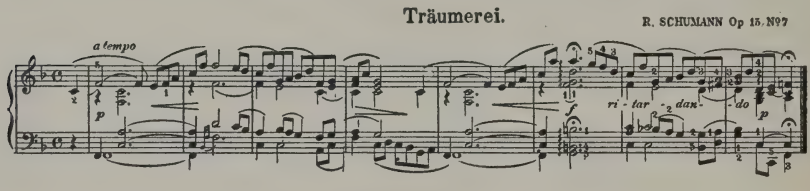
FIGURE 1
PASSAGE FROM SCHUMANN'S *Träumerei* WHICH WAS ADOPTED FOR PEDALLING PURPOSES

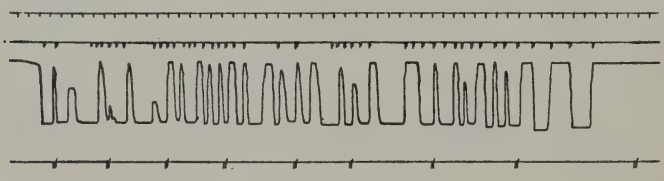

FIGURE 2
PEDAL KYMOGRAM OF SUBJECT A PLAYING FROM PRINTED SCORE

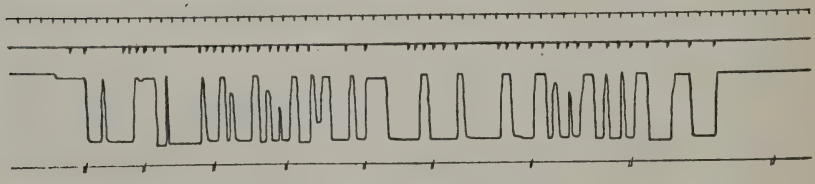

FIGURE 3
PEDAL KYMOGRAM OF SUBJECT A PLAYING FROM MEMORY

and 3). Subjects B and D employed both auditory and kinaesthetic imagery. They likewise did not entertain any visual images, nor did they give much attention to pedal action. Subject B, who is a professional musician, insisted that a good pianist, after once having thoroughly learned how to pedal, seldom, if ever, attends directly to his mode of pedalling, and that pedalling is in a large measure an automatic response. Although Subject C also employed auditory and kinaesthetic imagery in his memory performance, he depended for the most part on visual imagery, which he declared functioned throughout the rendition. He received a moving figure of the actual notation, and related this visual impression to the keys which he struck. In pedalling, he correlated depression with definite pitch points that were characterized by peculiar harmonic changes in the imaged series. It is to be recalled that this subject's memory performance closely approximated his sight-reading performance.

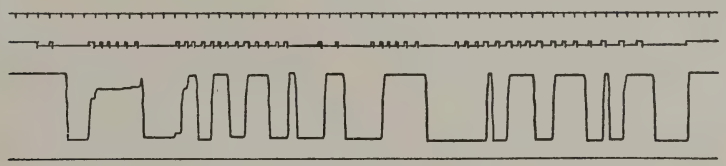

FIGURE 4
PEDAL KYMOGRAM OF SUBJECT A TAPPING FROM PRINTED SCORE THE MELODY ON AN ELECTRIC KEY WITHOUT TONAL EFFECTS

In the third task, each subject was requested to pedal the composition in exactly the same fashion as he had pedalled it in the first task, except that in this peformance, instead of playing from the keyboard which was now closed from his view, he was required to tap the melody on an electric key without tonal effects while reading the notated figure from the printed score. The electric key was the simple strap type which was held in the left hand at a comfortable position and depressed in unison by the index and middle fingers of the right hand. The subject was further instructed to tap the melody at the same tempo rate which he had chosen in the first rendition, and to indicate by continued depression of the electric key the originally adopted finger-sustension value of each melodic pitch-point.

The data obtained from this task are highly significant. No

subject was able to accomplish the requirements of the task, and the pedal patterns which resulted bore little or no relation to the pedal patterns of either of the preceding keyboard performances. Subject C became so perplexed in his attempt to correlate any type of pedal scheme whatsoever with his melodic tapping that he abandoned his performance at the end of the first phrase, so that it was necessary to obtain a second record. He found the second attempt as futile as he had the first. In his report he stated:

"Before I began, I felt sure that I would be able to recall my pedalling with ease, but somehow after the first measure I forgot everything. I believe I began beating out time with my foot. If I could have heard the music or touched the keys, I think I could have pedalled the same as I did before."

Subject A declared that the task was one of the most puzzling and difficult he had ever undertaken. In his initial performance he depressed the pedal 29 times representing his customary interpretation, whereas in this performance he depressed the pedal only 13 times. (For illustrative purposes, compare Figures 1 and 2 with 3.) Subject B remarked that after the first two measures, the task became so impossible that he merely guessed. Subject D, who required about 10 seconds longer than his first performance to complete this task, and whose pedal pattern in this instance bore no discernible relation to his initial rendition (his depression frequency ratio alone being 25:13), declared that despite the fact that he realized the approximate position of his previous pedal depressions on the score, his foot simply would not respond. He believed that had tones which corresponded to the notes of the printed page been sounded simultaneously with the depression of the electric key, it would have been much easier to accomplish the task. Subject A insisted that since pedalling to the musician was an inseparable part of a pianoforte rendition, it was impossible to give an intelligible pedal performance without actually playing the chords. Subject B remarked that he was unable to correlate his pedalling with the melody because he "missed the feel of the keyboard." Every subject accompanied his visual impression of the score with auditory imagery of both melodic contour and harmonic background. In no instance was there any trace of pedal shading effected in terms of half-release or half-depression. Total depression was the rule without exception. In the case of every subject, the time allowed to complete the composition was greater than the time allowed in the first rendition.

The instructions and requirements of the fourth task were like those of the third task, except that in this instance the subject was requested to perform from memory without the aid of the printed score. On the whole, this task proved more perplexing to the subjects than did the previous task. This task represented a pedal experience unlike any of those preceding.

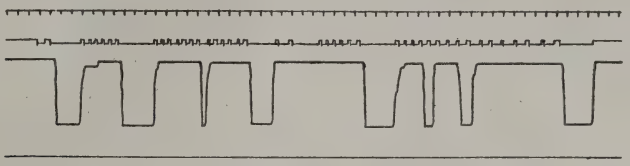

FIGURE 5
PEDAL KYMOGRAM OF SUBJECT A TAPPING FROM MEMORY THE MELODY ON AN ELECTRIC KEY WITHOUT TONAL EFFECTS

Subject A, who found it impossible to relate his pedal scheme with his initial performance, depressed the pedal only 8 times as compared with a pedal frequency of 29 in his first rendition. Although auditory imagery was declared to function throughout and, in general, to reach a high stage of vividness, it was found impossible to relate any adequate pedal scheme to the imaged musical progression. If some relation was momentarily discovered, it proved to be forced and artificial rather than spontaneous and natural. The two components, pedal pattern and imaged musical progression, were not integrated in consciousness as a total configuration or unity of effect. Whenever the attentive factor was shifted to the pedal responses, the imaged melodic figure tended to disintegrate; and, whenever the musical progression was sustained in the focus of attention, the pedal response suffered in consequence. In general, however, attention was chiefly directed towards the imaged musical progression, while the pedal was depressed at random.

Subject B, who achieved his greatest pedal frequency in this task, depressed the pedal 12 more times than he did in his previous performance. This frequency increase is attributed to the adoption of the melodic type of pedalling as the simplest method of emphasizing by explicit muscular movement the temporal relationships of the successively imaged pitch-points involved in the melodic progression. This subject, whose auditory imagery is exceptionally vivid, heard the bass figure in the fifth measure and responded, in

compliance with the type of pedalling adopted, with a pedal depression for each tone in that pitch region.

Subject C, who after the first two measures found it impossible to relate his pedalling with the melodic contour which he was imaging, as a means of escape resorted to rhythmic depression throughout the remaining measures. Subject D, who like Subject A was unable to discover any adequate relation between the two segregated elements through any single act of attention, simply pedalled at random without any effort to systematize response.

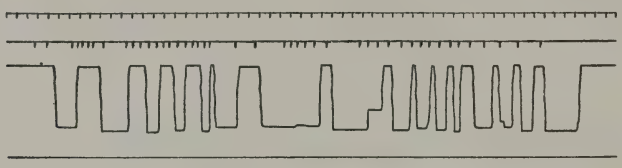

FIGURE 6
PEDAL KYMOGRAM OF SUBJECT A LISTENING WITH AID OF PRINTED SCORE TO GODOWSKY'S RENDITION ON THE DUO-ART

In the fifth task, each subject was again presented with the printed score as a visible guide and instructed to pedal, according to his usual and customary performance which he had adopted in the first task, the composition while listening to Leopold Godowsky's rendition as recorded on the Duo-Art reproducing piano (see and hear Duo-Art roll No. 5855-3). The pedalling was enacted without touching the keyboard which was closed to view.

In order to familiarize the subject thoroughly with Godowsky's interpretation of the particular excerpt of the composition which herein serves as the basis for comparative study, on a second piano (Duo-Art upright) which was placed at a right angle to the reproducing piano, the subject was allowed in two trials to play and to pedal simultaneously with the Duo-Art recording. It should be noted that the pedal action of the reproducing piano was eliminated by sealing the bar aperture which controls the damper mechanism and over which the pedal perforation of the recording roll passes. Thus, while the subject was able to familiarize himself with Godowsky's melodic phrasing, he was unable to gain any suggestion of the same artist's mode of pedalling. The staccato effect which resulted in many instances from the elimination of the damper mechanism on the reproducing piano was not manifest to

the subject during these preliminary trials, since the tonal sustensions introduced by his own rendition predominated and had a tendency to blanket the short durational effects produced by the roll at piano intensity.

While the first trial, because of the novelty of the situation and of the differences in phrasal interpretation experienced, offered some difficulty in an attempt to synchronize, every subject was able with considerable facility and accuracy to accompany the Duo-Art recording the second time it was rendered. These preliminary trials greatly assisted each subject to properly anticipate pitch incidents and melodic phrasing.

At the close of the second trial, the subject was recalled to the grand piano on which the reproducing roll was played and on which the subject recorded his own pedal interpretation of Godowsky's rendition. It should be remembered that in this task the subject was instructed to use the score as a guide and to keep his hands inactive by holding the sides of his chair.

In analyzing the records obtained from this phase of the experiment, no similarity in pedal pattern could be traced between the performance of this task and the performances of any of the preceding tasks. The most emphatic pattern deviations were made by the professional musicians. Every subject reported that while it was possible to anticipate melodic phrasing and pitch incidence, it was impossible to attribute to the presentation a type of pedalling that he would attribute to his own individual rendition. It was insisted by each that specific temporal value involving specific phrasal formulation demanded a specific type of pedalling. However, it is interesting to note that no one subject agreed with the other as to what form the pedal pattern ought to assume. The subjects unanimously agreed that the chief difficulty experienced in pedalling was due to the fact that they were not accorded opportunity to touch the keyboard. They declared individually that had they been given an opportunity merely to glance at the keyboard occasionally, that even this would have materially assisted them in pedalling.

The experimental conditions in the sixth task were practically the same as those in the fifth, with the exception that each subject was deprived of the notated score which had hitherto served as a guide. This task proved considerably more difficult than the pre-

ceding one. Without use of the score, it was found to be decidedly more difficult to anticipate tonal incidence and melodic phrasing. While the successive pedal patterns of Subject A and of Subject C evidenced many corresponding characteristics in regard to frequency, incidence, and wave-form, the successive pedal patterns of Subject B and of Subject D evidenced considerable variation in all characteristics with very little correspondence in form. In the case of Subjects A and C, similarity in repetition of

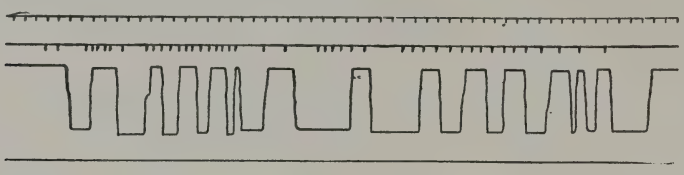

FIGURE 7
PEDAL KYMOGRAM OF SUBJECT A LISTENING WITHOUT AID OF PRINTED SCORE TO GODOWSKY'S RENDITION ON THE DUO-ART

response may be explained in part as the adequate function of immediate recall supported by a stimulus pattern practically identical in characteristics with the one preceding. The slight temporal variations in tenths of a second as indicated between Performances 5 and 6 in Table 1 may be explained as due either to alterations in paper tension following rewinding or to speed fluctuations of the pneumatic mechanism that motivates the recording roll. However, such fluctuations are so slight that in a complex tonal progression they are considerably below perceptibility (1).

In the seventh task, the subject was required, without touching the finger-board, to pedal from memory while singing or whistling the melody of the composition. It was first planned to have each subject whistle the melody. However, since neither Subject B nor Subject D could whistle, it was necessary to resort to the singing method in which the expression "la" was employed as the pitch symbol throughout the melodic range. Subjects A and C were able to whistle the composition without difficulty. During this performance, the keyboard was again concealed from view. Each subject was instructed either to whistle or to sing at the same rate which he had chosen in the first task.

While the rate chosen by each subject closely approximated his original or customary rate in keyboard performance, considerable variation followed in the method of phrasing. This condition of

phrasal variation is explained as being due to certain protensity inadequacies which the subjects experienced in either whistling or singing. Subjects A, B, and D resorted to auditory imagery throughout in which the melody was projected upon its proper harmonic background. Subject C made use of visual, auditory, and kinaesthetic imagery. Subjects A and D frequently depended upon mere guessing and stated that it was impossible to establish pedal points when keyboard hand position was omitted. Subject A declared that his ability to pedal correctly was largely dependant upon the position of his fingers on the keys—meaning thereby, apparently, that there existed an intrinsic relation between finger pattern and

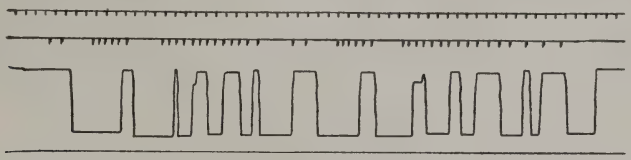

FIGURE 8
PEDAL KYMOGRAM OF SUBJECT A WHISTLING MELODY

pedal function. Subject C, despite the several forms of imagery to which he referred as aids, succumbed to "beating out time" with his foot, while Subject B, guided by considerable guess-work, fell victim to depressing the pedal every time he sang "la."

In the eighth task, the subject was requested to pedal in his customary manner (which he had established in the first task) while playing the melody from memory. The use of the right index finger only to strike every piano key involved in the melodic sequence eliminated the possible function of some pre-established finger pattern which might possess a definite relation to certain harmonic values and to pedal pattern. Again the subject was asked to adopt the rate that he would choose in his usual keyboard interpretation, and to sustain each melodic pitch-point for the same length of time that he would ordinarily sustain it were he to play the composition with both hands and to employ his customary fingering.

This task proved more difficult to the subjects than that immediately preceding. Subjects A and D, despite desperate efforts to avoid any synchronization of pedal depression with finger stroke, repeatedly found themselves helpless in this regard. All four subjects declared that the impulse to depress the pedal just as the key

was struck was almost irresistible. Subject C found this impulse so overwhelming that he was unable to avoid it and depressed the pedal, with only one exception, every time he struck a key. Subject B was the victim of a similar experience, although his control at times was slightly better. Both Subjects A and D produced pedal patterns evidencing odd characteristics of semi-depression and release (see Figure 9 and Table 1), a peculiar result which may be taken as an expression of their several endeavors to inhibit the impulse to which Subjects B and D surrendered.

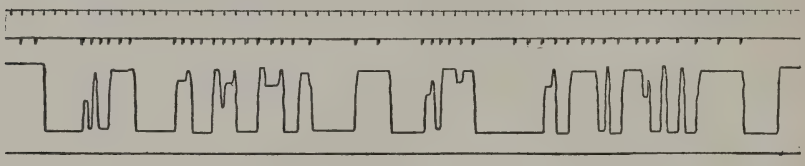

FIGURE 9
PEDAL KYMOGRAM OF SUBJECT A PLAYING WITH THE RIGHT INDEX FINGER THE MELODY FROM MEMORY

In the ninth task, each subject was asked to close his eyes, to image auditorily the composition as if he were actually playing it, and to employ his customary pedal interpretation during such imagery. The pedal patterns resulting from such an imaged experience were so diverse in form that they are presented for simple inspection and comparison in Figures 10 to 13, inclusive. Because of the nature of this task, it was impossible to obtain any objective record of pitch incidence or method of phrasing. Each subject reported that he employed visual and kinaesthetic imagery as well as auditory imagery during the pedal performance. In regard to similarity of this pedal pattern with any of those preceding, the pattern of Subject A bore greatest resemblance to his pattern of

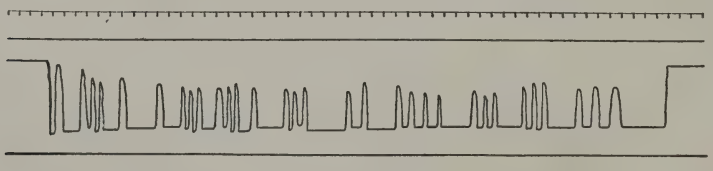

FIGURE 10
PEDAL KYMOGRAM OF SUBJECT B IMAGING COMPOSITION AS IF BEING PLAYED BY HIMSELF

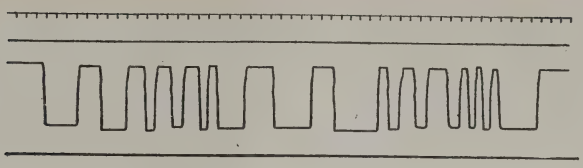

FIGURE 11
PEDAL KYMOGRAM OF SUBJECT A IMAGING COMPOSITION AS IF BEING PLAYED BY HIMSELF

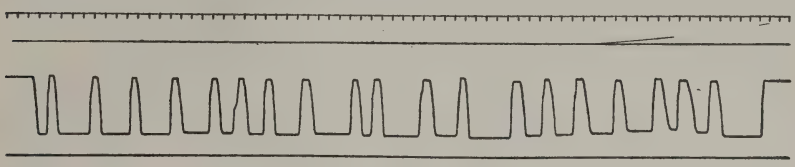

FIGURE 12
PEDAL KYMOGRAM OF SUBJECT C IMAGING COMPOSITION AS IF BEING PLAYED BY HIMSELF

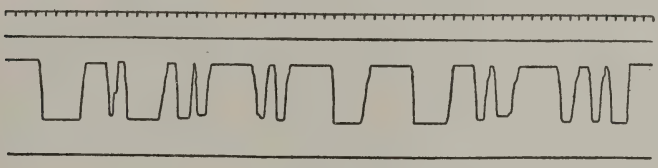

FIGURE 13
PEDAL KYMOGRAM OF SUBJECT D IMAGING COMPOSITION AS IF BEING PLAYED BY HIMSELF

the sixth task, while the patterns of Subjects B and C bore greatest resemblance to their respective patterns of the seventh task. The pedal pattern of Subject D bore little resemblance to any of his preceding patterns.

In the tenth and final task, the subject was required, without touching the keyboard, to pedal from memory while listening to a rendition of the same excerpt as recorded on the Panatrope[3] by the Cleveland Symphony Orchestra (Brunswick New Hall of Fame Series, No. 15091). Subject B was not available for this task.

In order to familiarize the subjects with Nikolai Sokoloff's inter-

[3]The Panatrope offers several positive advantages for scientific investigation. This instrument, unlike the old type of resonance machine, provides superior quality reproduction. A magnetic pick-up of the arma-

pretation of this passage, two preliminary hearings of the record were given with the rheostat adjusted to moderate intensity. The phonographic reproduction offered to each subject an entirely new type of experience through the introduction of the various orchestral timbre effects. In addition to this timbre novelty, unlike the transient nature of the pianoforte tonal sustension, the orchestral presentation is characterized by tonal duration that is practically continuous and overlapping for successive pitch-points from the beginning of the composition to the close. It was this persistent and

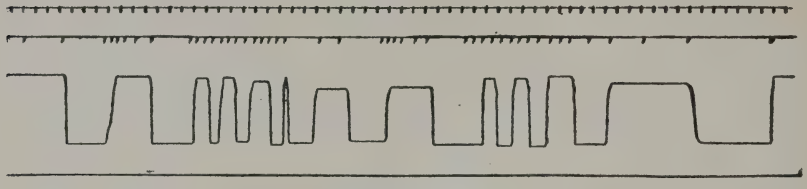

FIGURE 14

PEDAL KYMOGRAM OF SUBJECT A LISTENING WITHOUT AID OF PRINTED SCORE TO A RENDITION OF THE COMPOSITION AS RECORDED ON THE PANATROPE BY THE CLEVELAND SYMPHONY ORCHESTRA

uninterrupted tonal sustension that forced each subject to abandon any attempt to relate his customary pianoforte pedal interpretation to the orchestral scheme. Each subject's choice of the harmonic pedal pattern as the most adequate form in this instance was, for the most part, dependent upon the peculiar psychological effect produced by the broad, sweeping, well-sustained chords voiced by the many fusing timbres into which the melodic contour seemed to merge. None of the pedal patterns in this task bore any identity to those which preceded.

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ture type converts the disc frequencies into electrical oscillations which are transferred to a dynamic speaker through a bank of non-distorting amplifiers. An accurate vernier control affords a large range of intensities. Alternating current rectifiers of the vacuum tube type employed in conjunction with a scratch filter, balanced potentiometer, and ballast tube circuit, maintain an approximately constant intensity coefficient and aid in the elimination of cyclic interference. The turntable is rotated by an induction motor of the disc armature type which maintains constant speed during passage of the diaphragm from the record periphery to record center. Radiotron UV-876 provides a practically constant voltage influx despite any fluctuations in electromotive pressure in the circuit main.

The writer wishes to thank the Brunswick Company of Baltimore, Maryland, for its kind cooperation and loan of the Panatrope employed in this experimentation.

Conclusion

The damper-pedal performance of a well-trained pianist is essentially an automatic response which has been conditioned by, and is functionally dependent upon, finger pattern, finger progression, keyboard position, method of phrasing, speed of rendition, variation in tonal intensity and in timbre, extent of tonal anticipation, degree of tonal sustension, type of imagery employed in recall, and appreciation of and vividness of attention to tonal relationships. The damper-pedal response is so highly integrated into a unity of effect with the other phases of a pianoforte performance that alteration of any single factor or group of factors mentioned above may seriously modify a pianist's customary pedal interpretation. Increase in musical proficiency seems to be marked by an increase in the difficulty experienced to avoid such damper-pedal pattern modification.

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L'USAGE DE LA PÉDALE FORTE DU PIANO DANS DIX CONDITIONS EXPÉRIMENTALES DIFFÉRENTES

(Résumé)

Les résultats d'une enquête expérimentale sur quatre réacteurs indiquent que l'usage de la pédale forte par un pianiste bien instruit est essentiellement une réponse autamatique qui dépend de la forme du doigter, de la progression du doigter, de la position sur le clavier, de la méthode de faire les phrases, de la vitesse de l'interprétation, de la variation de l'intensité tonale et de celle du timbre, de l'anticipation tonale, du degré de soutenir les tons, du type des images employées dans le rappel, de l'appréciation des relations des tons et de l'intensité de l'attention qu'on y fait. La réponse est tellement liée aux autres phases d'une exécution musicale que l'altération d'un seul facteur on d'un groupe des facteurs cités en haut peut modifier gravement l'usage usuel de la pédale par un pianiste. L'amélioration de l'exécution musicale semble marquée d'une augmentation de la difficulté d'éviter une telle modification.

Heinlein

ÜBER DEN GEBRAUCH DES PEDALS BEIM KLAVIERSPIEL UNTER ZEHN VERSCHIEDENEN UMSTÄNDEN

(Referat)

Die Ergebnisse einer experimentellen Untersuchung an vier Vpn. beweisen, dass der Gebrauch des Pedals bei einem wohlgeschulten Pianisten wesentlich eine automatische Reaktion ist, die von dem Fingersatz, der Fingerfolge, Stellung auf den Tasten, Methode der Phrasierung und des Nachdrucks, Tempo des Spiels, Variation der Tonstärke und der Klangfarbe, Lebendigkeit der Tonvorahnung, Anhalten des Tones, Typus der Phantasie bei Wiederholungen, Verständnis und Lebendigkeit der Tonbeziehungen abhängt. Die Reaktion ist so innig mit den andern Phasen des Klavierspiels verbunden, dass die Veränderung eines einzigen Faktors oder einer Gruppe der oben erwähnten Faktoren, den gewohnten Pedalgebrauch des Spielers stark beeinflussen kann. Grössere musikalische Geschicklichkeit erhöht nur die Schwierigkeit—wie es scheint—solch nachteilige Wirkung zu vermeiden.

HEINLEIN

THE GALVANIC PHENOMENON AND CONDITIONED RESPONSES*

From the Psychological Laboratory of Cornell University

G. L. FREEMAN

The psychology of the "conditioned reflex" often meets with difficulty when translated into a working program. The technique originally developed by Pavlov is not especially applicable to human subjects, although several successful studies on children have been reported by Lashley (5), Krasnogorski (4), and others. A substitute method introduced by Bekhterev (1) uses certain motor responses as the indicator of conditioning. While these methods have met with favor among experimentalists, even their strongest advocates admit certain limitations. The foremost of these limitations lies in the fact that only those conditioned responses can be recorded which are traceable to a specific group of muscles; whereas common observation points to the fact that the great majority of conditioned responses which the normal adult makes are too complex to reduce to the contractions of a single muscle or group of muscles or glands. Any new method proposed for the study of behavior of this type must also take into account the fact that the inhibitions of the human individual tend to reduce all overt manifestations to a minimum. The sudden flash of a red light in the path of the motorist does not, for example, generally throw him into a state of frightened frenzy or paralyzed fear. The emotional response is largely inhibited and our method must be delicate enough to record the vestiges of these natural and general responses.

The galvanic phenomenon obtained by passing a small ex-somatic current through a subject in series with a galvanometer offers the possibility of recording minute disturbances of the total organism in experiments of the conditioned type. Golla (2) has reported a case where a subject's conditioned response of a general character was recorded by this method. After giving a series of 13 electric shocks, he found that a subject gave a galvanic response to a series

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of clicks normally preceding the shocks, even though the shock was not given. The work of Wells (7) with the galvanic phenomenon bears evidence of a conditioned response made to a slightly different type of situation. Her subjects came to respond to the sound of a timing device immediately preceding the moment when a "choice" was to be demanded. The author, however, does not interpret this fact as a conditioned response. A similar situation holds for a number of other studies upon the galvanic phenomenon which I have reviewed. Looked at from one angle, the critical responses in the familiar association-reaction test might also be regarded as conditioned. I am further informed[1] that the galvanic phenomenon has been successfully used to exhibit conditioned responses in a least one undergraduate course in this country. So far as I have been able to ascertain, however, no frontal attack has been made on the problem. This paper reports two short experiments which seem to point to the feasibility of this method of recording conditioned responses.

General Technique

In these experiments I undertook to condition the galvanic response elicited by an electric shock to a series of clicks immediately preceding the shock. The recording apparatus was set up after the method of Wechsler (6) in which the subject is attached to the fourth arm of a Wheatstone bridge. Before every experiment the *S* is put, by means of switches, in series with the galvanometer and the amount of current flowing through the circuit is regulated by a potentiometer. Since the bodily resistance varies not only among individuals but also in the same individual at different times, it is claimed that this method of adjusting the amount of current to produce a standard deflection makes possible a comparison of individual records. Readings of the deflections can either be taken directly from the mm. scale or the moving point of light can be focused on the slit of a panoramic camera and photographed.

Two undergraduates, F and L, were used in the experiments. Since it seemed advisable to keep these subjects in complete ignorance of the nature of the problem, a false assignment was given to

[1]From private correspondence with S. Hathaway.

them.[2] They were asked to continue a "memory" experiment in which they had served as subjects a short time previously. On each arm of the chair in which the *S* sat there were two clay vessels the *S* placed the index and middle finger of each hand in these vessels. The two vessels on the right side were non-polarizable electrodes connected with the secondary of an induction coil. A single dry cell was used in the primary circuit. The current from the secondary was, however, fairly intensive and was delivered on the "make" to both of the immersed fingers. Two experiments were performed and at the conclusion of each one the subjects were asked to remark upon the experience.

Experiment I

Conditioned and Unconditioned Responses

The *S* placed his fingers in the vessels; he was told to relax and sit still, and that he would be given a short series of nonsense syllables to repeat as soon as the experimenter "got the apparatus adjusted." When the adjustment had been made, the time apparatus was started. The latter produced a clicking sound which served as the "conditioned" stimulus. Exactly five seconds after the beginning of these sounds a shock was delivered to the fingers of the left hand. On the first trial both subjects reacted to the shock by withdrawing their left fingers from the vessels. They were told that this movement greatly affected the apparatus and were asked to refrain from withdrawing their fingers in case they got the shock again. Both immediately threatened to quit the experiment unless the shocks could be eliminated. They were told that this might not be possible and were finally cajoled into finishing "today's series at least."

After each trial the *S* was shown a short series of nonsense syllables and told to repeat the syllables to himself. With much difficulty F was persuaded to sit through 24 trials and L through 18. The galvanic responses of both subjects to both clicks and shocks are given below in mm. of deflection. In a few cases, marked by the asterisk, the shock was not given.

[2]The design of this general arrangement was to encourage the subjects in their conclusions that the shocks, which served as the "conditioned" stimuli, resulted from bad technique. I. M. Hamel (3) mentions the experimental difficulties encountered with adults who have gained an insight into a problem of this type.

TABLE 1

SHOWING MM. DEFLECTION TO CLICKS AND SHOCK FOR TWO SUBJECTS
(Arranged in serial order of trials)

Trial	Subject F		Subject L	
	Clicks	Shock	Clicks	Shock
1	0	79.2	0	16.3
2	0	92.7	97.1	69.3
3	64	66.6	45.8	27
4	0	52.3	40	123
5	75	66.7	86.4	37.2
6	78	27.3	42.5	33.8
7	12.8	16.5	38.1	27.5
8	23.6	77	0	32.6
9	53.7	69.7	63.3	22.7
10*	57	0	27.6	26.9
11*	0	0	21	16.7
12*	0	0	0	0
13	0	75.9	0	115.1
14	0	64	74.2	46
15	55.2	69.7	57	30
16*	47.1	32.3	24.3	0
17	0	53	0	111
18	0	47	56.2	76.8
19	38.5	27.3		
20	32.6	20		
21	26.3	43.7		
22*	12.2	0		
23	0	53.8		
24	43	0		

*Cases where shock was not given.

Table 1 shows that Subject F responded to the clicks in the third trial, but not in the fourth. The responses to the clicks in Trials 5, 6, 7, 8, and 9 indicate, however, that by this time the conditioned response is established. While the response fails to appear in Trials 11 and 12, when the reinforcement of the unconditioned stimulus was removed, it is quickly re-established in Trial 15. The removal of the shock again in Trial 16 breaks down the response until Trial 19. The response then persists until the shock is once more withheld (Trial 22). In this last case, however, it reappears more quickly than before (Trial 24).

Subject L also responds to the sound of the clicks almost immediately (Trial 2). The conditioned responses persist until Trial 8, when no deflection was obtained. The response reappears in greater intensity in Trial 9, however, and also occurs in both Trials 10 and 12 where the shock was not given. In these latter cases dual con-

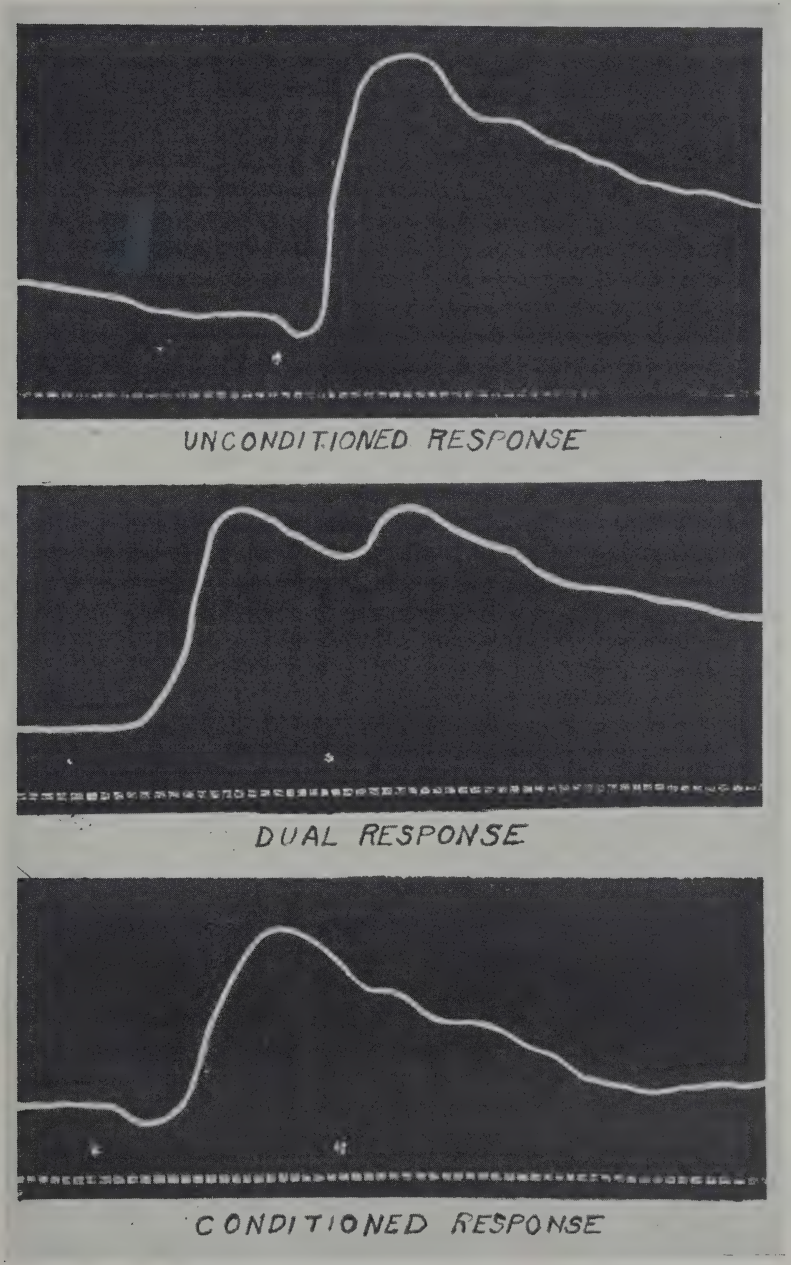

FIGURE 1
SHOWING TYPICAL RESPONSES FOR SUBJECT L
Time markings four per second. The white dots above the time line denote the moment when the stimulus was given

ditioned response is recorded, i.e., L has responded not only to the original "conditioning" stimulus but also to the expectation of the exact moment when the shock is usually applied. Both these responses fail to appear in Trial 12, but the primary conditioned response is again elicited the second time that the original stimulus is given (Trial 14); it disappears in Trial 17, following the failure to give the shock in Trial 16, only to reappear in heightened form in the next trial (18).

Three of L's records, which were photographed, are shown in the accompanying cut. The first (Trial 8) shows a response to the shock alone, i.e., an unconditioned response in isolation. The second (Trial 11) shows responses to the clicks and to the expectation of the moment of the shock, i.e., a dual conditioned response. The third (Trial 16) shows a response to the clicks alone (the shock was not given), i.e., a primary conditioned response in isolation.

My interest in the experiment was primarily technical rather than psychological. I desired, however, to know something of the experiences of the subjects, and, since they were purposely misled concerning the nature of the experiment, I could not give a formal instruction to report. Consequently I merely asked, "How did you stand the experiment?" The following typical comments are taken from their replies:

L. I came always to expect a shock. Once or twice I thought that you had the apparatus fixed so that it wasn't going to bother any more. Sometimes the shock was not as severe as at other times. I think that when my expectation was greatest, I noticed the shock less.

F. Along toward the end I began to think that there was something "fishy" about those shocks. It seemed that I got a shock only when I wasn't expecting it.

The above reports suggest the possibility of a different type of explanation from that given in this paper. Since the quantitative results were obtained in a manner similar to that of animal experimentation, I have interpreted them as "conditioned responses"; but it seems that they might just as easily be interpreted as physiological accompaniments of the attitudes of expectation and surprise. Certainly it seems clear that the attitude of expectation was the psychological correlate of the "conditioned response" in the present experiment; and there is some direct evidence for the inference that the larger deflections made in response to a shock after its absence in a preceding trial (see Trials 13, 17, 23) were correlated with

the attitude of surprise. Further, if it were possible to apply the psychological interpretation of non-expectancy to Trial 4 of F and Trial 8 of L, their anomalous character would disappear. In order to make such an explanation valid, experimental procedure would have to be shaped with the experiential aspect primarily in mind. Should subsequent experiment, however, support the presence of expectation in the human subject as the psychological correlate to "conditioned" behavior of the type recorded, the method here employed might not only be useful in the psychological study of the attitude, but we would also have a basis for inference to the psychological aspect of animal behavior under similar conditions. In this connection it is interesting to note that the greater response of Subject L to the conditioned stimulus than to the unconditioned, a frequent observation in the motor reflexes of animals, was attributed by him to expectation.

Experiment II. Discrimination

In this experiment I tested the possibility of recording discrimination by the galvanic response. Such a procedure, in the terminology of the method, involves the conditioning of the original response to a positive stimulus in the face of a negative one. If, when the subject is conditioned to one stimulus (positive), another stimulus (negative) somewhat like it is presented, the *S* may or may not respond, according as he "discriminates" between the two stimuli. In experimental practice the original stimulus always follows the "positive" but never the "negative." The timing device used in Experiment I could be speeded up or slowed down at will, resulting in either a slow or a rapid series of clicks. A slow series (4 per second) was chosen as the negative, and a rapid series (12 per second) as the positive stimulus. The subjects were told that the apparatus was now "fixed" and were asked to continue the memory experiment. Contrary to established procedure, the negative stimulus, without acompanying shock of course, was given in the first two trials. Thereafter the positive rate was sounded and then followed by the shock. The negative rate was introduced only occasionally to test for discrimination. The exact order of positive and negative stimuli presentations appears in the table below. Every trial was followed by a short series of nonsense syllables as in Experiment I.

F was dismissed after the 19th trial when he stated that he was

sure I controlled the shocking device. He said, "At first I really thought that when the machine speeded up some short circuit caused the shock. Now I doubt if it is the fault of the machine." This idea never seems to have occurred to L. He refused to continue after the 26th trial until I had fixed the apparatus "so it won't race."[3]

The quantitative results of the discrimination experiments are shown in Table 2 below.

TABLE 2

SHOWING THE MM. DEFLECTIONS OF TWO SUBJECTS TO POSITIVE AND NEGATIVE STIMULI, AS WELL AS TO THE ORIGINAL STIMULUS (SHOCK)

Trial	Subject F			Subject L		
	Positive	Negative	Shock	Positive	Negative	Shock
1		0			0	
2		0			0	
3	0		97.2	0		103.5
4	0		83	85.2		88.3
5	84.2		63.7	76		75.7
6	75		41.6	81.3		60.5
7		67			73	
8		0			52	
9	59.8		62.3	24.3		96.7
10	51		37.2	72		53.2
11		0			64.1	
12	30		59.9	42.7		50
13	48.3		40.3	88.9		32.7
14	45		36.5	65		51.2
15	48.2		86.7	43.7		47.8
16		0			0	
17	53.1		75.8	88		87.2
18	69		44.3	73.1		80.6
19		0			0	
20				50		88.4
21				53.8		72.3
22					0	
23				67.5		60
24					0	
25				75.3		72.7
26				90.9		56.4

The table above shows that Subject F came to discriminate between the positive and the negative rate by the 7th trial; in Trials 8, 11, 16, and 19 no response was made when the negative stimulus was given. Subject L, on the other hand, cannot be said to have learned to discriminate until Trial 15; in Trials 7, 8, and 11 he responded

[3]Both subjects were assured that the matter had been beyond my control. They were told that they need not report again until the mechanician had corrected the fault of the apparatus.

positively, although the negative rate was not followed by the shock. The fact that the *S*'s again reported expectation indicates that experiential factors are just as prominent in discrimination as in the case of the establishment of the conditioned response in Experiment I.

Conclusion

While these two experiments are far too few in number to warrant any sweeping conclusions as to the general utility of galvanic technique in the study of conditioned responses, the positive character of our results indicates its practicability. Not only has a conditioned response been consistently exhibited by means of the technique in Experiment I, but the method has also proved adequate to the objective study of discrimination in Experiment II. Since the galvanic technique has been used in the study of the totally integrated physiological functions of the organism (respiration, pulse, muscular activity, etc.), and since the general organic disturbances in emotional states and certain of the bodily "postures" involve the activity of all these functional systems, the galvanic phenomenon has proved most useful in the study of the general physiological activity of the organism during these states. It is a common observation that a variety of stimuli become the "signal" for fear, expectation, etc. Our method of using the galvanic technique makes it possible to exhibit conditioned behavior of this type through the vehicle of the physiological activity involved in the response to the original and to the conditioned stimulus.

The procedure of giving a false instruction to the subject, while making for a more naïve attitude toward the experiment, is questionable. I am still in doubt whether it is better to give the false assignment, or to trust that the subject will not question the purpose of the experiment and thereby complicate the setting. In the latter case a more careful choice of subjects might lessen the chance of failure; a good observer never questions the experimenter's purpose. Certainly, should the galvanic phenomenon be used in the study of expectation as I have suggested, the necessity of direct instruction to report on the attitude would prohibit the giving of a false assignment.

Summary

Two short experiments are reported which indicate the practicability of using the galvanic phenomenon as an indicator of condi-

tioned responses in human subjects. While no great claim can be made for the method until it is put to more rigorous test, it would seem to hold certain advantages over other methods in that it apparently registers the response of the total organism. It is to be regretted that the subjects would not continue the experiments over a longer period of time. Their remarks made during the experiment indicate that expectation was the psychological correlate of the type of conditioned response recorded. Such a suggestion not only offers a method for studying this psychological attitude, but it opens also a possible basis of inference to the psychological aspect of conditioned behavior in animals.

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LE PHÉNOMÈNE GALVANIQUE ET LES RÉPONSES CONDITIONNELLES

(Résumé)

La psychologie du "réflexe conditionnel" rencontre souvent de la difficulté quand introduite dans un programme expérimental. La technique développée premièrement par Pavlov n'est par très applicable aux sujets humains et le méthode alternative de noter quelque réponse motrice spécifique a ses désavantages. Cette étude-ci présente deux courtes expériencés dans lesquelles lé phénomène galvanique a été employé avec succès comme indicateur du comportement conditionnel. Dans l'Expérience I deux sujets, qui ont d'abord donné des réponses galvaniques à un choc électrique seul (stimulus non conditionnel) ont fini par répondre à une série de bruits secs (stimulus conditionnel) précédant le choc. Dans l'Expérience II les réponses galvaniques ont montré que les sujets ont *discriminé* entre deux vitesses différentes des bruits secs, c'est-à-dire, une réponse a été établie pour une grande vitesse des bruits (stimulus positif) par opposition à une plus petite vitesse (stimulus négatif). On a rapporté l'expectation comme le corrélat psychologique de la réponse conditionnelle.

Ces deux expériences indiquent la practicabilité d'employer le phénomène galvanique dans l'étude des réponses conditionnelles chez les sujets humains. Bien qu'on ne puisse louer sans réservations la méthode jusqu'à ce qu'on l'ait mise à une épreuve plus rigoreuse, le fait qu'elle semble montrer une réponse de l'*organisme entier* suggère certains avantages sur ces méthodes limitées aux réponses somatiques spécifiques. La même méthode pourrait être de valeur aussi dans une étude psychologique de l'expectation.

FREEMAN

ÜBER GALVANISCHE ERSCHEINUNGEN UND BEDINGTE REAKTIONEN

(Referat)

Die Psychologie des bedingten Reflexes stösst oft auf Schwierigkeiten wenn sie auf experimentelle Untersuchungen angewendet wird. Die ursprüunglich von Pavlov entwickelte Technik eignet sich nicht sonderlich für menschliche Vpn. und die alternative Methode eine besondere motorische Reaktion zu verzeichnen hat gewisse Nachteile. Die vorliegende Studie beschreibt zwei Experimente in denen die galvanische Erscheinung mit Erfolg als "Indikator" des bedingten Verhaltens angewendet wurde. Die zwei Vpn. in Experiment I, die zuerst nur auf einen elektrischen Schlag galvanisch reagirten (unbedingter Reiz), reagirten späterhin auf eine Serie von "clicks," i.e., kurze, scharfe Töne (bedingter Reiz), die dem elektrischen Schlag vorhergingen. In Experiment II zeigten galvanische Reaktionen dass die Vpn. zwei verschiedene Tempo der Tonserie *unterschieden,* i.e., eine Reaktion folgte auf ein schnelles Tempo der Tonserie (positiver Reiz), im Gegensatz zu einem langsamern Tempo der Serie (negativer Reiz). Erwartung wurde als psychologisches Korrelat der bedingten Reaktion bezeichnet.

Diese beiden Experimente beweisen die Anwendbarkeit der galvanischen Erscheinung zum Studium der bedingten Reaktionen beim Menschen. Obgleich man noch nicht zu viel zu gunsten der Methode sagen darf ehe dieselbe gründlicher geprüft worden ist,—der Umstand dass dieselbe scheinbar eine Reaktion des *ganzen Organismus* verzeichnet, deutet gewisse Vorteile über solche Methoden an, die auf besondere somatische Reaktionen beschränkt sind. Nebenbei bemerkt möchte sich dieselbe Methode zum psychologischen Studium der Erwartung als wertvoll erweisen.

FREEMAN

SHORT ARTICLES AND NOTES

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NOTICE OF SPECIAL VOLUME

CARL MURCHISON

Along with most of the other psychological journals, we have been facing for sometime the problem of diminishing the large number of manuscripts which have been accumulating steadily in our files and which have resulted in lengthening unduly the amount of time necessary to get a manuscript published after it has been received from its author. This problem has been met in various ways by some of the other psychological journals, and we have finally decided on a method which to us seems wisest under the circumstances.

We have decided to publish an entire volume in one issue, this issue to appear between two current volumes. This method will result in the saving of an entire year and will clear our files to within approximately six months of publication. We have decided on this method chiefly because it adds no additional financial burdens to the author and extends to all authors alike equal benefits in the reduction of the length of publication time.

This volume, Volume 4, will not be sent out on subscription, nor will it be involved in the Club Rate plan. It must be ordered directly by each subscriber at the regular subscription price of $7. Volume 5 will be issued as the current volume during the year 1931.

BODY POSTURE AND GEOTROPIC RESPONSE IN THE ANT *APHAENOGASTER FULVA*

T. CUNLIFFE BARNES

We have recently described[1] a progressive increase in the extent of geotropic orientation exhibited by the ant *Aphaenogaster fulva* when confined away from the colony for a few days; we have shown that this rise in response exhibits a rectilinear relation with time. We have successfully

[1]To be published shortly; this was chiefly the work of my colleague Mr. B. F. Skinner.

eliminated the possible influence of previous locomotion both on the level and on an inclined surface, for the same phenomenon is exhibited by individuals exercised daily on the incline, or on a level, or merely kept in a moist watch-glass without opportunity for crawling.

In the communication alluded to, we put forward an hypothesis which attempted to explain the rather striking increase of geotropism on the grounds of "muscular weakness," and we wish to present further evidence on this head in the present paper. Professor Crozier, whose guidance has been quite indispensable in all our tropism studies, suggested that we should measure the body posture and extension of the legs of an *Aphaenogaster* during the period of progressive change, for such a change in the disposition of the limbs of a species with long legs such as *Aphaenogaster* might change the relative position of the center of gravity and thus influence the geotropic response.

A change in posture quite independent of frequency of leg movements has been established as the cause of phototropic response in *Ranatra* by Crozier and Federighi (3); a somewhat similar phenomenon occurs in young rats (4); and it is quite evident that no description of tropistic response is complete which is lacking in careful measurements of concomitant postural reactions. It need hardly be said that the significance of maintained body posture for tropisms is quite distinct from the study of the *Korperstellung* of the stationary animal as pursued by Magnus (5). A maintained and symmetrical posture-change and its effect upon tropistic sensitivity affords a third type of postural response.

Individual ants of *Aphaenogaster fulva* were kept in small cells of a few cubic centimeters surrounded with dry cotton and a marked extension of the legs was observed after a day's treatment. Death followed in a few hours, but some preparations were able to crawl upon an inclined plane. Specimen No. 80 with a spread of five millimeters between the tarsi of the two metathoracic legs (measured on a straight line at right angles to the body) gave an angle of geotropic response six degrees higher than that of a fresh ant, on a 50° inclination of the substratum. (The hind tarsi of the control ants were not more than 3 mm. apart.) This increased response might follow from the mechanical advantage of the extended limbs in crawling up the plane, or from a greater number of proprioceptors stimulated in the stretched muscles. Such individuals were placed in a moist chamber and the posture of the legs returned to the normal state, but the increased geotropic response was still evident.

When individuals are kept in isolation in a watch glass with a moist sponge the rise in the geotropic response is quite gradual, but no corresponding changes in the body posture can be detected. The individual whose geotropism is represented in Figure 1 was examined for postural changes at frequent intervals. Each point in Figure 1 is the average of 26 paths on a surface with constant inclination of 50°. It will be noted that

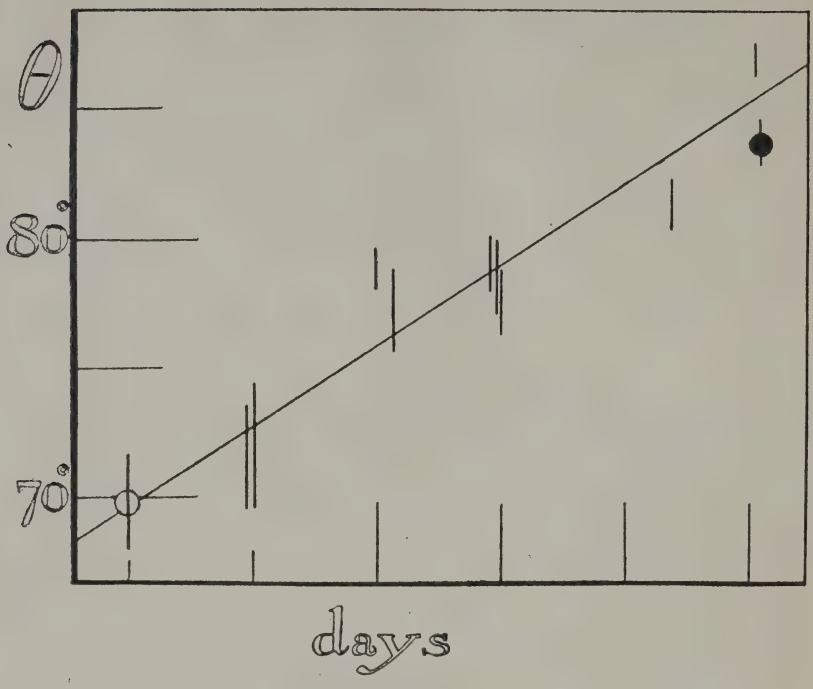

FIGURE 1

The gradual rise in the geotropic sensitivity of an individual of *Aphaenogaster fulva* taken from a fresh populous and vigorous colony. The increase in the extent of gravity-response exhibits a rectilinear relation with the time in isolation as previously reported, but the sensitivity is more than six degrees of θ higher than with the same species from an old laboratory nest. The bars representing the points on the curve are equal to 2 *P.E.*; the lowest point is taken from an individual fresh from the nest; the black point is that of the control animal which was kept without crawling on the plane for the same time; the white point is the geotropic response of a typical ant fresh from the nest.

variation exists between adjacent points which was not evident in the figure published previously, due to the larger number of paths then measured. In Figure 3 the extensions of the legs of this individual are recorded. There is no change in the placement of the limbs at the end of the fourth day when the progressive rise in θ is almost at its height. The distance from the tip of the tarsus to the coxa was measured for each leg when the animal was at temporary rest on a substratum ruled in millimeters. Many determinations were made but one is not justified in averaging a large number of measurements, for the position of the hind limbs

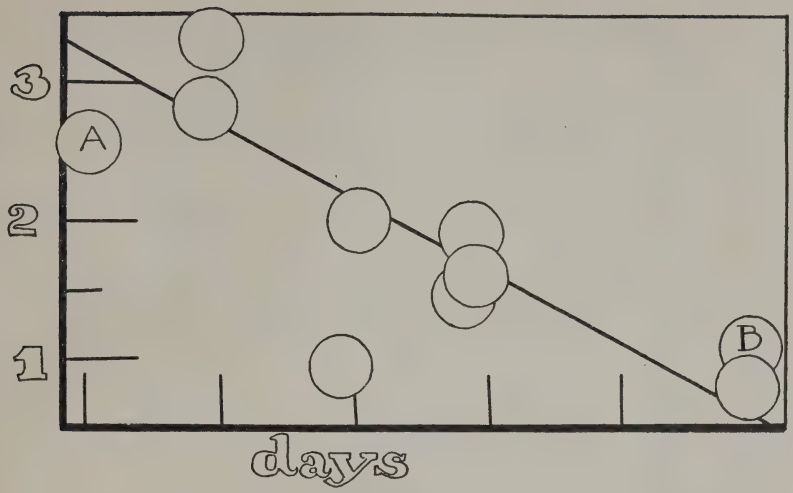

FIGURE 2

The coefficient of variation (100 x Probable Error divided by the mean angle of geotropic response, Θ) is plotted against the time of confinement in days as abscissae. This is the same individual ant, *Aphaenogaster*, represented in Figure 1. The variability of Θ decreases with time.

of a given individual is partially controlled by the placement of the fore legs. This is especially true in the record for the end of the third day (*vide* Figure 3).

Further corroboration of our "muscular weakness" hypothesis was obtained in a study of the variation in the geotropic response. The coefficient of variation, expressed as 100 x *P.E.* divided by the mean, falls as the progressive rise in Θ takes place (Figure 2), which would be consistent with a loss in vigor.

After solitary confinement without food[2], the Θ angle of geotropism is higher not because the proprioceptive sensoria are more sensitive with a given degree of tension but because the turning to one side or the other which ordinarily functions against the tendency to adjust the asymmetry of proprioceptive stimulation is greatly reduced in the weak animals. It would appear that the Θ angle does not represent merely the geotropic sensitivity, but also the degree of resistance depending on the vigor of the animal.

The ant of Figures 1 to 3 exhibited signs of weakness on the fourth day and at times reclined on one side when standing (Figure 4, G); it would

[2]When groups of two or three are isolated without food the same rise occurs, in spite of the mutual "preening." Ants live if returned to the colony in time.

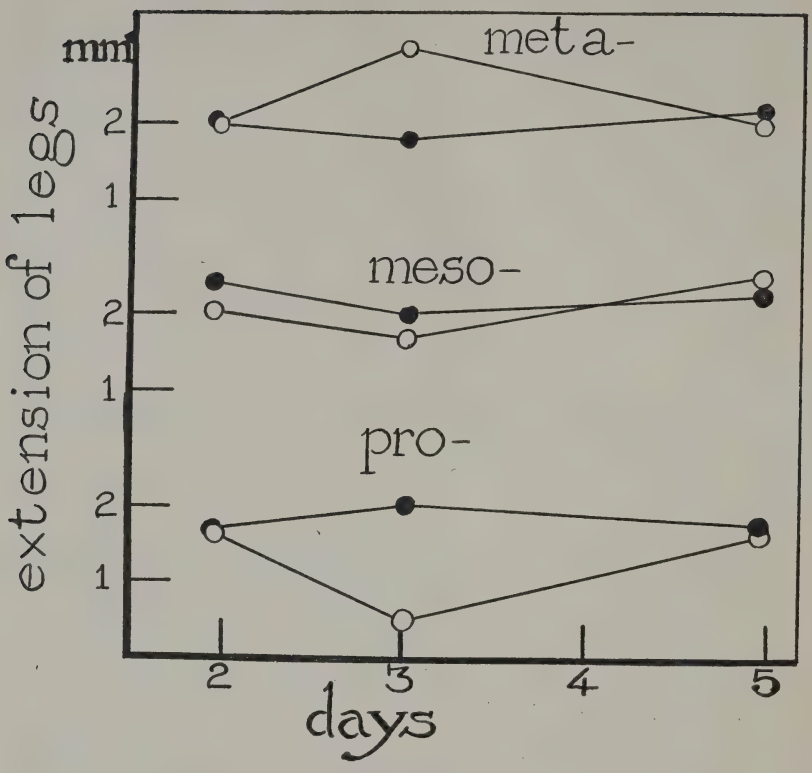

FIGURE 3

The extension of the legs of the individual ant (*Aphaenogaster*) whose geotropic response is recorded in Figures 1 and 2. The distance, from the tips of the tarsi of the prothoracic, mesothoracic, and metathoracic legs to the distal termination of the respective coxae was measured on the ant while standing on a substratum ruled in millimeters. These measurements were made during the days when the progressive rise in the geotropic response occurred. The white circles represent the right legs and the black dots the left legs. No systematic change in posture is exhibited.

crawl rapidly if disturbed and escaped from the watch glass on the fifth day. The drawings in Figure 4 were made from two specimens during a temporary pause and are accurate to 0.2 of a mm. *Aphaenogaster fulva* is supported from the center of its mass (Figure 4*A* and *B*), and this posture explains the previously reported fact[3] that a weight added to the head (a pupa carried) acts as additional weight and increases the Θ angle. The

[3]See footnote 1.

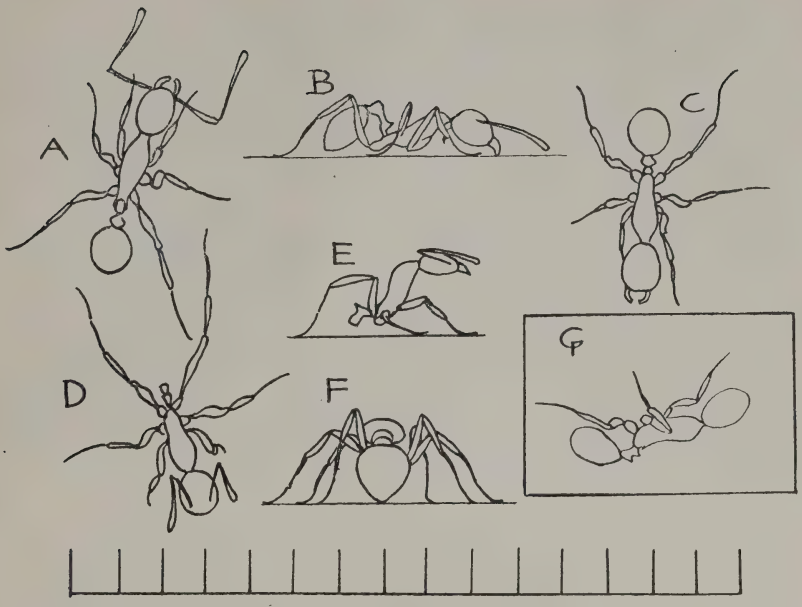

FIGURE 4

Body posture of the ant *Aphaenogaster fulva.* All these drawings were made through the microscope; the ant was permitted to attain a natural posture upon a surface ruled in millimeters (mm. scale at foot of figure). The diagrams are accurate to 0.2 mm.

A. Aphaenogaster No. 88, whose geotropic responses may be seen in Figures 1 and 2, and whose posture is measured in Figure 3; vertical surface, 20 June 11:35 A.M. on the second day of confinement. Note that the legs spring from the center of the animal.

B. The same individual drawn from a side view on a horizontal surface against a background of millimeter squares. The animal is in temporary rest; when in locomotion the mandibles are not resting on the substratum; note that the gaster is supported from the center of the insect.

C. Aphaenogaster No. 89. Antennae removed; horizontal surface; the prothoracic legs are thrown forward to replace the antennae sensoria; this altered posture would tend to move the center of gravity forward but any consequent modification of the geotropic response is quite entirely masked by the tremendously increased oscillations from side to side described in a previous paper (2).

D. Aphaenogaster No. 87. Abdomen removed; insect in temporary resting posture on a vertical surface; the Θ angle of such ants is a bit reduced due to the loss in weight.

E. Aphaenogaster No. 87. Drawn from the side; standing on a horizontal surface; three hours after ablation of gaster; the thorax is raised and the center of gravity moved backwards possibly to compensate for the loss of the weight of the missing abdomen; the decrease in the Θ angle of

such preparations is not more than five degrees at $\alpha = 50°$.

F. Aphaenogaster No. 89, antennae removed half hour previously; no change in posture of meso- and metathoracic legs; the tip of the abdomen is resting on the horizontal surface but is carried when in locomotion; note that the legs support the animal at the center of the body as indicated in *B*.

G. Aphaenogaster No. 88, whose geotropic record is represented in Figures 1-3, June 24, 1929, on the fourth day of confinement; the ant is "reclining" on a horizontal surface; only the left legs are drawn; the "muscular weakness" is evident.

abdomen also springs from the center and is carried a bit forward (not as the grotesque textbook figures portray). Ablation of the gaster (*D*) causes a drop in the Θ value but the body is thrown back in such preparations (*E*) which partially compensates for the loss in weight. The gaster sometimes rests upon the ground (the mandibles also), but never when the ant is crawling (*B, F*). Ablation of the antennae, which produces an enormous drop in the Θ angle due to the exaggerated turning of the ant from side to side, produces little change in leg posture (*C*); the fore legs are slightly more extended, as if to replace the antennae.

Summary

Evidence is brought forward that the progressive rise in the geotropic response of the ant *Aphaenogaster fulva* tested on a constant angle of inclination of the substratum is not due to postural changes of the legs. A number of body postures are described and their tropistic significance is discussed in reference to results given in earlier papers.

TABLE 1

Records of the Progressive Rise in Θ. (vide Figure 1 and 2)

	Day	Θ *P.E.* (degrees)	$\frac{100 \times P.E.}{\text{mean } \Theta}$ (degrees)	Time
$50_{(1)}$	1	71.5±2.01	2.81	20 June 12:02-12:13
$50_{(2)}$	1	72.0±2.40	3.33	20 June 1:12- 1:22
$50_{(3)}$	2	78.8±0.77	0.97	21 June 1:07- 1:24
$50_{(4)}$	2	77.2±1.60	2.07	21 June 4:26- 4:45
$50_{(5)}$	3	79.0±1.18	1.49	22 June 12:09-12:14
$50_{(6)}$	3	78.5±1.52	1.93	22 June 12:26-12:34
$50_{(7)}$	3	77.5±1.25	1.61	22 June 12:45-12:50
$50_{(8)}$	4	81.3±1.05	1.29	23 June 8:50- 8-55 P.M.
$50_{(9)}$				
$50_{(10)}$	5	86.9±0.71	0.81	24 June 1:50- 1:58
			94	(high-control)
$50_{(1)}$	5	83.7±0.93	1.11	27 June 2:46- 2:56
			102	(low-control)
$50_{(1)}$	0	69.8±1.80	2.57	1 July 4:43- 4:51

TABLE 2
EXTENSION OF LEGS OF *Aphaenogaster*
(See Figure 3)

Date	Formula in mm.					
	R_1*	L_1	R_2	L_2	R_3	L_3
21 June 12	1.6	1.7	2	2.4	2	2
22 June 1	.5	2	1.7	2	3	1.8
21 June 12	1.7	1.8	2.5	2.3	2	2.2

*R_1=1.6 signifies that the right fore leg is 1.6 mm. in extension from the coxa to the tip of the tarsus.

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BOOKS

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FLETCHER, H. *Speech and Hearing.* New York: Van Nostrand, 1929. Pp. 331.

The field of auditory theory has long been in need of a fresh supply of experimental data. Controversy and discussion have been for a considerable while based in large part upon speculation arising from consideration of facts established years ago in experiments carried on with apparatus which definitely limited the precision of the results. In recent years remarkable advances in the development of apparatus necessary for precise investigation of auditory problems have been made in the laboratories of sound transmission. The result has been some very careful experimentation which furnishes us with precise information in place of the approximations which had to suffice for so many years. Fletcher's *Speech and Hearing* is the result of investigations carried on by several members of the staff in the laboratories of the Bell Telephone System, and is of great value to any one interested in problems of speech and hearing. The fundamental urge back of the researches was no doubt practical and the problems were apparently chosen with the demands of telephonic communication in mind. However, this has not limited the investigation to problems whose solutions are of immediate practical importance. The technical information contained in this book will be of great value to all people who are interested in the problems of sound and hearing. The influence of telephone engineering practice is apparent in the terminology used throughout the book and the expression of auditory values in terms of speech powers is at times confusing. A considerable part of the terminology is new and it is to be hoped that much of it will be generally adopted. The use of the term *cycle* in place of double vibration is an example of usage which might profitably prevail.

Part I, consisting of four chapters, deals with the general subject of speech.

A short discussion of the formation of speech sounds and a description of apparatus for the artificial production of speech sounds make up Chapter I. Chapter 2 includes description of the methods of recording speech waves. Both the methods which have only historical interest and those made possible by the invention of the vacuum tube receive adequate attention. Discussion of typical speech waves, theories of vowel production, and general speech characteristics conclude Chapter 2. Chapter 3 deals with the general subject of speech powers and includes definitions of several of the terms which are used throughout the book. A short chapter on the frequency of occurrence of different speech sounds concludes Part I.

Part II is given over to a consideration of the properties of musical sounds and noises. A method and the apparatus for the analysis of tones from musical instruments is described. Discussion of the acoustic spectra of typical musical instruments and the ranges for frequency and intensity for music concludes the first chapter. A chapter on noise includes consideration of a method of measuring noise and the results of noise surveys.

Part III, consisting of 6 chapters, deals with the general subject of hearing. Chapter 1 is a clear and concise presentation of the facts relative to the structure and function of the auditory mechanism. The author accepts an extension of the theory proposed by Helmholtz as the most likely theory of hearing and discusses it at some length. It is his belief that the evidence is overwhelmingly in favor of this theory and he gives scant attention to other types of theory. He suggests that an explanation of the experience of loudness is to be found in the conditions of nerve conduction and discusses at length the importance of recent work upon nerve function for auditory theory. Chapter 2 treats with the limits of audition and describes the methods and experiments by means of which the data presented were collected. Along with the "threshold of audibility," a new concept the "threshold of feeling" is introduced, which corresponds to the practical upper intensity limit of human hearing. A page and a half of the book is given to discussion of pitch limits, and 20 to 20,000 cycles per second is given as the average range of pitch sensitivity. Minimum perceptible differences are treated in Chapter 3 which opens with a definition of the Weber-Fechner law. An interesting array of data upon minimum perceptible differences of intensity and frequency does not seem to offer very clear support for the law, and the author concludes that "the Weber-Fechner law does not hold rigorously." Chapter 3 ends with a discussion of the positions on the basilar membrane of the nerve terminals which mediate the perception of pure tones. Chapter 4 treats with the masking effects of pure tones and complex sounds. The brief discussion of subjective tones is suggestive in many ways. Chapter 5 devotes 10 pages to a discussion of binaural beats and the localization of sound images. Methods of testing the acuity of hearing are dealt with in Chapter 6.

Part IV, consisting of 82 pages, is devoted to the discussion of the perception of speech and music. The loudness of sounds is discussed in Chapter 1. Chapters 2 and 3 deal with the recognition of the pitch of musical tones and methods of measuring the recognition of speech sounds. The effect of changes in the received intensity of speech sounds and the effects of frequency distortion upon recognition are dealt with in Chapters 4 and 5. Chapters 6 and 7 deal with the effects of several types of distortion and the effect of noise and deafness upon the recognition of speech sounds.

Six appendices treating such subjects as the calibration of a condenser transmitter by means of a thermophone, the application of Hooke's law to the transmission of sound in the inner ear, and the derivation of equations concerning properties of a plane wave bring the book to an end. Adequate references are given throughout the book and the index is especially satisfactory in its completeness.

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HOLLINGWORTH, H. L. *Psychology: Its Facts and Principles.* New York: Appleton, 1928. Pp. xviii+539.

The author's several articles on redintegration, the psychophysical continuum, the higher thought processes, and the like, together with his books, *Mental Growth and Decline* and *The Psychology of Thought,* find a logical sequel in *Psychology: Its Facts and Principles.* It is designed as a text, which aims to organize the facts of psychology about the principle of redintegration, without committance to the assumptions of structuralism, the visual bias of behaviorism, the mechanistic difficulties of associationism, and the "several vices" of Gestalt psychology.

The book is long, heavy with abstruse philosophical and logical assertions, and replete with criticisms of conventional psychology. For these reasons a brief review of it will give the reader only an inadequate conception of its contents.

To begin with, the author's philosophical conception of the psychophysical continuum leads him to a distinction between mental processes and subjective events—the two sets of subject matter with which psychology is concerned. The first three chapters are devoted to preliminary material, such as definitions and methods. Redintegration is asserted as the criterion of mind. The subjective and the objective are construed as the consistency or privacy with which events are reported, without regard to the patterns exhibited in their sequence.

In Chapter 4 are presented the general laws of redintegration, which

are 15 in number. We can give only three as examples: (1) Consequents may follow fragments or partial details of their former antecedents. (2) Some of the details of an antecedent context have special instigative potency. (4) A mind is the sum total of redintegrative activities exhibited in or by a given system of events. It can be seen by the author's use of the terms, mind and subjective, that it is clearly his intention to avoid any dualistic separation of the mental from the physical. Chapter 5 is devoted to experimental data on redintegration, in which he brings the conditioned reflex and the phenomena of adaptation into relation with his basic principle. Chapter 6 is given over to "subjective" redintegrations (the rarer and peculiar phenomena which disagree with the average individual's experiences), such as synaesthesias, number forms, and sensory aurae.

Perhaps as clear a description of redintegrative sequences as appears anywhere in the book is to be found in Chapter 7, in which 14 of the laws are illustrated, one by one, in the use and meaning of words. The next four chapters, 8 to 11, are devoted to perception; and the following three, 12 to 14, deal with learning and memory. By perception is meant the interpretation of "natural signs," that is, "objects" as opposed to language symbols. The "natural sign" perceived is always a part of a certain antecedent complexity (stimulus-pattern). By learning is meant those activities or conditions whereby a cue (remnant of previously functioning stimulus-pattern) acquires instigative potency.

The principles of redintegration are next applied, in Chapter 15, to imagination, dreams, and invention. Here we are told that the criterion of memory is the pattern of an earlier antecedent, and that the criterion of imagination is the potency of the pattern in evoking a present or deferred consequent, such as pleasure, or satisfaction. The details of the imaginative pattern (symbols) occur in *new* organization of *old* elements.

Then follow chapters on the consequents and dynamics of mental activity—motivation, drives, habits, instincts, and cravings. The author is cautious in the use of the term instinct and relegates much to habit that the earlier literature defined as instinct. Such activities as seem definitely to be instinctive are referred to as patterns of bodily structure (intra-organic stimulation).

With somewhat of a break in continuity, the author jumps next to a chapter on reasoning, whose laws are still those of redintegration. The outstanding conditions that figure in this mode of behavior are the richness of past experience, the delicacy of response to subtle cues, and the potency of details to function for their past contexts. Several illustrations are drawn from introspective literature to bear out the thesis.

Attention is attacked next, which is treated as a redintegrative organization in which the details of stimulus-patterns exhibit hierarchies of potency. Finally, almost as after-thoughts, the last two chapters have

to do with the senses and intelligence. Then follow several appendices designed to amplify the test and to assist the student in his study of it.

It is very difficult to evaluate a book of this sort. It is difficult reading and is evidently designed for advanced classes, especially in systematic psychology. As such, it is suggestive and stimulating; but so unconventional and abstruse, and so replete with logical rather than descriptive analysis that its merits must be judged as an individualistic, consistent, and scholarly attempt at system-building rather than as a product with a general appeal. It is, withal, a refreshing departure, however, from the conventional dualistic systems and those which are based upon the mechanistic and associationistic efforts to build up complex behavior upon simple elements of response. While the reviewer does not share the author's respect for redintegration as an all-embracing principle, he does regard *Psychology: Its Facts and Principles,* from the standpoint of its scholarliness, as one of the outstanding books of the present.

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WELD, H. P. *Psychology as Science: Its Problems and Points of View.* New York: Holt, 1928. Pp. xi+297.

In *Psychology as Science* Professor Weld, for years a colleague and companion of the late Dean of American Experimentalists, Edward Bradford Titchener, has put into condensed form and has extended the application of the principles for which Titchener was so well known. Not that Professor Weld has written a systematic text as Titchener would have written it, for the book bears unmistakable marks of individuality; yet, it carries on the Titchenerian traditions.

A proportionately large amount of space is given over, in the earlier part of the book, to a distinction between the logical approach to and the fundamental problems of scientific psychology. First, as regards the meaning of science, the author adheres to the classical view that science is concerned with description and classification. Prediction and control are relegated to technology and to "empirical science." It naturally follows, from this position, that empirical psychology (functional and behavioristic) is not science, in the critical sense of the term, because of its aims to explain, to predict and to control. This means that it has no particular method of its own aside from common sense methods; and it affords no logical distinction between scientific facts and facts of everyday life.

In contrast with this type of psychology, there grew up—beginning, perhaps, with Mach and Avenarius, and coming down through Wundt, James Ward, Külpe, and Titchener—a new viewpoint, and ostensibly a

new method, which is called *existential psychology*. This type of psychology claims to meet the demands of critical science, because (1) its facts are meaningless, in that they do not refer beyond themselves; they are truly impersonal; (2) its problem is description; (3) its method is that of observation of the "given," and avoids the errors of common sense interpretative observations (?); and (4) its goal is a knowledge of the existential world, for its own sake. So much for the discussion of general psychology.

The nature and problems of differential psychology are discussed next. Its problems rise out of variations in the nervous system, and may be divided into three groups: (1) problems of variation—range and distribution of traits, relation of traits to each other, and personality; (2) conditions of variation—heredity, education, social status, sex, and age; (3) the variety of ways in which variations are expressed—handwriting, gesture, literary style, and the like. This entire field, according to Weld, is undeveloped, from the standpoint of critical science. It has suffered from the crudities, inconsistencies, and ambiguities of empirical science. There is hope, however, that when existential psychology attacks these problems, the situation will be improved.

Three chapters are now devoted to the psychology of the abnormal, which, systematically, is considered a phase of differential psychology. Here again, work with the abnormal has been done chiefly from the standpoint of empirical psychology, with its emphasis upon meaning, with the result that progress has been impeded. The problem in this field is to correlate neural processes with meaning and behavior. While this is what the "empiricists" have been trying to do, they have failed to recognize the intermediate correlation between meaning and sensory process. When this bridge has been constructed "many of the puzzling problems of the psychology of the abnormal will disappear."

Then follow four chapters dealing with problems in animal and comparative psychology. Here, again, the author contrasts the fruitfulness of the empirical as opposed to the existential viewpoint. The logic with which he brings animal psychology into the existential fold is this: The experimenter who observes the animal's "report" may, from his psychological knowledge, interpret the behavior of the animal as a report of existential experience. Then, as distributive facts, having a certain degree of probability, the results may be brought into relation with human descriptions of experience. Of course, there is a great deal of material in this field which belongs, by definition, to empirical psychology, as for example, learning, habit, instinct, and the like. The best that the existential psychologist can do is to look for evidences of sensations, imagery, and ideas. Similarly, problems in mental development are confined, in existential psychology, to qualitative experience, its multiplication,

increase in range or intensity, duration and clearness, the acquisition of imagery, and an increase in the number and complexity of the integrations of existential processes.

Since social organization and all the cultural products of society may be regarded as expressions of psychological experience, we are left with the impression, in the absence of a direct statement, that perhaps social psychology is not entirely outside the boundaries of critical, psychological science.

In his concluding chapter, the status of psychology, the author argues for a close cooperation between those who are interested in description and those who are interested in explanation, to the end that the facts of psychology, of whatever kind, may be integrated into an intelligible synthesis.

The book is exceedingly well written, in delightful style, and with splendid organization. In spite of the fact that it deals with distinctions to which many a reader will not be sympathetic, no critic can judge it other than a most valuable presentation of an important viewpoint. While it may not make converts, therefore (which is probably not the author's intention), it will help preserve to future psychology a movement whose influence either directly or indirectly did much, and will do much to render psychology genuinely scientific.

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PAPEZ, J. W. *Comparative Neurology.* New York: Crowell, 1929. Pp. xxv+518. (315 figures.)

This book is especially noteworthy for its excellent figures illustrating vertebrate nervous systems, and for the systematic labelling and indexing of the figures, thus greatly facilitating comparative study. The text is especially adapted to use as an instructor's laboratory manual, but also contains much that is of value to the elementary student.

There are three main divisions. Part I discusses the gross structure of the brains of mammals. Part II considers the microscopic structure of mammalian nervous systems, and Part III considers the brains of the lower vertebrates. The text is primarily concerned with anatomical description and comparison, and one is sometimes a bit disappointed with the lack of physiological discussion. There are, however, several chapters dealing with physiological material. A good review is presented of work on reflexes of postural tonus, and the physiology of the cerebellar cortex and of the cerebral cortex are treated in separate chapters.

The chapter on the physiology of the cerebral cortex contains a review

of much of the classical work and is well illustrated; it is not, however, especially up to date. One is surprised to find no mention whatsoever of Lashley's experiments on functional localization or of Head's recent views of aphasia. There is a good but brief review of Pavlov's work on conditioned reflexes.

The book is to be judged on the merits of its anatomical description, since the physiological aim of the author is obviously a secondary one. The amount of anatomical material dealt with is very extensive but is made digestible by the wealth of figures. The book is to be highly recommended as a reference book of comparative vertebrate neurology.

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RUCKMICK, C. A. *The Mental Life: A Study of Modern Experimental Psychology.* New York: Longmans, Green, 1928. Pp. ix+253.

The Mental Life was written for the Longmans' Psychology Series and for the purpose of satisfying a twofold demand: first, for a condensed summary of the field for use in summer sessions and in normal schools; and, secondly, for a supplement to other texts.

This book is written, in general, along the line of Wundtian and Titchenerian traditions, although it deviates from them in relatively unimportant particulars. Its content is confined largely to descriptive material dealing with "experience," and little is said about prediction and control. Commencing with definitions, and a statement of different kinds of psychology, it proceeds to the "basis of experience" which is found in sensation. The qualities of sensation are discussed first, followed by a treatment of the "related aspects of sensory experience"; namely, adaptation, contrast, after-image, fusion, and intensity relations. Then comes a chapter on perception, which opens with a discussion of meaning. Among the three or more factors upon which meaning is based are (1) "inherited or instinctive tendencies, physiologically buried in our nervous systems due to racial experience," (2) previous individual experience, and (3) the associative trends of the moment. Qualitative percepts, spatial percepts, temporal percepts, and percepts of movement are then presented briefly. Following the chapter on perception, there is a chapter devoted to "imaginal experience," and then one devoted to affective experience.

The next chapter deals with attention, which subject receives conventional treatment for the most part. Then comes a chapter on association. Attention is treated from the standpoint of "attensive organization of experience," while association is treated as a "protensive" organization. One paragraph, devoted to *Gestalt* psychology, comes in the chapter on association under the paragraph heading, "The Psychology of Form." The problems of memory

and thinking are also presented in the same chapter. Learning, instinct, habit, and will are then touched upon in a chapter on action. The book ends with chapters on the thought processes and the self.

On the one hand, the book is very carefully written in an easy, pleasant style. It claims to make no contribution other than by way of assembling elementary material in a way that may meet certain pedagogical needs. The statements of principle are made clear by adequate examples; the author's frequent allusions to the history of psychology lend meaning to the problems that are discussed. On the other hand, it must appeal, or not, according to the reader's attitude toward conventional sensationistic psychology. The book rests upon the old plan of building up from the simple to the complex, i.e., experience is based on sensations, and action is based on reflexes. There is almost nothing said about method, and very little new material is presented. It gives almost no hint of the transition stage through which psychology seems certainly to be passing. However, from the standpoint of organization and style, it reads like a very teachable book.

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